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LOGICAL ANALYSIS OF GESTALT CONCEPTS *

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I *Introduction*

THIS paper attempts to construct a conceptual framework for the precise explication of Gestalt concepts, such as those of whole, Gestalt-quality, and system, as they are employed in various branches of empirical science. Our starting point is given by the terms *part* and *whole*. Even a superficial survey of the relevant literature shows that these are the basic concepts for our investigation. The part-whole concept is a very general one, accommodating a wide variety of usages. This fact is attested to by the diversity of functions which the terms 'part' and 'whole' play in everyday and in technical parlance.[2] However, in the present study we will not confine ourselves to some particular sense of 'part' and 'whole'; rather, we wish to retain the wide range of application of these terms. To assure such breadth of applicability, we here assume that only certain essential features of the part-whole relation are taken as fixed and specified, and all else left open as a matter of application and interpretation.[3]

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[1] This paper has as point of departure various studies by K. Grelling and P. Oppenheim, of which the published work we shall have occasion to cite constitutes only a portion. The writers have greatly benefited by discussions with C. G. Hempel. The responsibility for any faults is, however, exclusively theirs.

[2] The reader is referred to E. Nagel's article, 'Wholes, Sums, and Organic Unities', *Philosophical Studies*, 1952, **3**, 17-32, for an illuminating survey of the more important of these senses.

[3] The broad range here to be allowed for the interpretation of 'part' and 'whole' might be assured by specifying a *set of axioms* for the part-whole relation, providing a formal abstract characterisation of this relation. Thus it would be only in the context of a particular part-whole relation providing a valid interpretation of the given axiom system, that one would speak of a 'whole' or a 'part'. And an object which is a 'whole' in one sense of the concept of part-whole might fail to be a 'whole' in some other sense.

It should be stressed—as essential to an understanding of motivation—that in scientific discourse the application of the terminology of 'part' and 'whole' is generally governed by underlying criteria; criteria which are, admittedly, not fully articulated in formality and precision. Consider Köhler's example of what one might regard as the very antithesis of a 'true' whole: three stones, selected at random, lying in different continents. Although each of the stones is, to be sure, a part of the group of three, this group is not, on the usual understanding of the term, a whole.[1]

Let us attempt to summarise the intuitive requirements or conditions of adequacy which underlie talk of *wholes*. To begin with, the things we are considering as *wholes*, must, of course, consist (in some sense) of *parts*. In addition, it would seem that the following conditions must be met:

(i) The whole must possess some *attribute* in virtue of its status as a whole—an attribute peculiar to it, and characteristic of it as a whole.
(ii) The parts of the whole must stand in some special and characteristic *relation* of dependence with one another; they must satisfy some special condition in virtue of their status as parts of a whole.
(iii) The whole must possess some kind of *structure*, in virtue of which certain specifically structural characteristics pertain to it.

The remainder of this paper is devoted to an analytical study of these three conditions.

2 *Attributes of Wholes*

Let us first consider item (i) in the list of conditions for such wholes. We shall here examine species of the genus indicated by the vague notion of a property peculiar to, and characteristic of, the whole as such, in contrast with its parts.

The basic set of axioms of the part-whole relation should not be such as to disallow interpretations which represent the accepted usages of 'part' and 'whole' in everyday or in scientific discourse. A set of axioms which provides a suitable formal basis for the present discussion is presented in an article by N. Rescher, 'Axioms for the Part Relation,' *Philosophical Studies*, 1955, **6**, 8–11.

[1] The term *aggregate* has been employed to characterise such failures to qualify under the intuitive notion of a 'true' whole. Actually, the term 'aggregate' has as many senses as there are criteria composing the intuitive notion of a whole: for failure to meet certain of these criteria yields a corresponding type of *aggregate*.

As we have noted above, to specify some concept of part we must state the conditions under which an object x is to be considered as a *part* (in this sense) of an object, or 'whole', y. The specification of a particular part-whole relation thus determines for a given object, or whole, w, just which objects are its parts, i.e. for what objects x it is the case that $xPtw$. It is only when the context makes clear which specific part relation is intended that we may speak of 'parts' and 'wholes' without further ado. Now, given a particular object, w, and some specific part relation, Pt, we will say that a class D of Pt-parts of w (i.e. parts as specified by Pt) is a *decomposition* of w if every Pt-part of w has some Pt-part in common—i.e. overlaps—with at least one element of D. Clearly, the class of all Pt-parts of an object is a decomposition in this sense, but the object may have other decompositions which do not include all of its Pt-parts.

We can turn to the definition of the first species here to be considered of the notion of a property peculiar to a whole as such.

Definition 1.1

An attribute[1] Q is a *D-unshared attribute* of a whole w relative to a decomposition D of w into Pt-parts, if Q is an attribute of w which is possessed by no Pt-part of w belonging to the decomposition D.

Relative to a given set of its parts, D, a D-unshared attribute of a whole is an attribute which, in fact, is not possessed by any one of these parts. If a pile (w) of round stones (D) has the attribute (Q) of being conical, this shape is an instance of a D-unshared attribute.[2]

Definition 1.2

An attribute Q is a *D-shared attribute* of a whole w relative to a decomposition D, if Q is an attribute of w which is possessed by all of the Pt-parts of w belonging to the decomposition D.

Relative to a given set D of its Pt-parts, a D-shared attribute of a whole is one which, in fact, is possessed by *each one* of these parts. (It is thus clear that an attribute which is not D-shared need not be

[1] In this paper the term *attribute* is used in its most general sense, and is intended to include qualitative properties, relational properties, numerical functor characteristics (e.g. age, weight, temperature), etc.

[2] The concept of a D-unshared attribute is essentially the same as what has been called a 'collective property' by C. D. Broad. 'The "Nature" of a Continuant' (reprinted in H. Feigl and W. Sellars' *Readings in Philosophical Analysis*, New York, Appleton-Century-Crofts, 1949, pp. 474-475).

D-unshared.) The attribute of having weight (*Q*) possessed by a pile (*w*) of stones (*D*) is a *D*-shared attribute of the pile.

Definition 2 . 1

An attribute *Q* of a whole *w* is a *D-G-underivable attribute* of *w* relative to a decomposition of *D* of *w* and to a set *G* of attributes if ' *Q*(*w*) ' is not a logical consequence of the characterisation of the elements of *D* with respect to *G*. By ' characterisation of the elements of *D* with respect to *G* ', or briefly ' *G*-characterisation ', is meant a sentence which, for any *n*-adic relation *g* of *G*, and any *n* elements $d_1, \ldots d_n$ of *D* states whether or not the relation holds between these *n* elements.[1]

In the case of a *D*-*G*-underivable attribute, it is not possible to discover by logical means alone that *w* possesses *Q* from the information (relative to *G*) about the parts of *w* (belonging to *D*). Thus, for example, the weight (*Q*) of a pile (*w*) of stones (*D*) is a *D*-*G*-underivable attribute of the pile relative to the weights (*G*) of the constituent stones. This is because the weight of the pile is not a *logical* consequence of the weights of the constituent stones, since its calculation from these requires a physical law (viz. that weight is additive).

Definition 2 . 2

An attribute *Q* of a whole *w* is a *D-G-derivable attribute* of *w* relative to a decomposition *D* of *w* and to a set *G* of attributes if ' *Q*(*w*) ' is a logical consequence of the *G*-characterisation of the *D*-parts of *w*.

Thus *Q* is a *D*-*G*-derivable attribute of *w* if it is possible to discover by logical means alone that *w* possesses *Q* from the information (relative to *G*) about the parts of *w* (belonging to *D*). For example, the *shape* of a pile of stones is a *D*-*G*-derivable attribute of the pile relative to a characterisation of the stones in the pile with respect to certain geometric attributes (including metric relations of position and distance). On the other hand, the shape (i.e. physical, not phenomenological) of the pile will be a *D*-*G*-underivable attribute if *G* is confined to attributes of the individual stones, exclusive of their mutual spatial relationships.

We have mentioned that the weight of a pile of stones is not a logical consequence of the weights of the constituent stones ; in fact, knowledge of the resultant weight is forthcoming only if some appropriate

[1] The explicit relativisation with respect to *G* is necessary to exclude trivialities from the applications. For instance, no property *Q* of a whole *w* with part *p* is underivable if ' being part of a whole that has *Q* ' is an admissible property of *p*.

scientific information is given. This fact provides the background for the definition :

Definition 3 . 1

An attribute Q of a whole w is a *D-G-T-underivable attribute* of w relative to a decomposition D of w, a set G of attributes and a theory T, if ' $Q(w)$ ' is not deducible by means of T from the G-characterisation of the D-parts of w.

The word ' theory ' as used in this definition requires some comments. In using this word we do not wish to rule out the possibility that T includes non-theoretical information, such as specific descriptive statements about the whole and its parts. The purely theoretical part of T will consist, in the scientifically interesting cases, of some portion of established science, i.e. a system of laws which at a given time are well established. *D-G-T*-underivable attributes are closely linked, as we shall now see, to what is usually discussed under the heading of *emergence* and therefore the case in which the theoretical part of T is a microstructure theory is especially relevant. (On this point see p. 151 in C. G. Hempel and P. Oppenheim, ' Studies in the Logic of Explanation', *Philosophy of Science*, 1948, **15**, 135-175.) Indeed, our definition of *D-G-T-underivable attribute* is adapted from the definition of ' emergence ' given by Hempel and Oppenheim (loc. cit.). Accordingly, emergence in this general sense has been termed *explanatory* emergence by G. Bergmann (p. 211 of ' Holism, Historicism, and Emergence ', *Philosophy of Science*, 1944, **4**, 209-221).

Because of the importance of the concept, it is convenient to refer to *D-G-T*-underivable attributes simply as (suitably relativised) *underivable attributes*. For example, let w be an atom of any element, D its subatomic particles, G their presently known properties, and T present-day nuclear physics. On this basis, w's possession of a cohesive nucleus is an underivable attribute.[1] As appears from this example, the underivable attributes are of especial scientific interest because they provide open problems for research.[2]

[1] For a popular discussion of this matter see H. A. Bethe's ' What holds the Nucleus Together ? ', *Scientific American*, 1953, **6**, 58-63.

[2] In this connection it is of interest to observe that one point of heated contention in the contemporary philosophy of biology is the question of the existence of biological attributes of organisms which are underived attributes relative to (1) the molecular structure of the organism, (2) to attributes dealt with in the physical sciences, and (3) to the physical sciences themselves—whether in their present or in *any possible* future state. From this interpretation of the *vitalist* position, it is clear that it represents no strictly scientific theory, but is at best of the status of a belief.

Definition 3 . 2

An attribute Q of a whole w is a *D-G-T-derivable attribute* of w relative to a decomposition D of w, a set G of attributes, and a theory T if ' $Q(w)$ ' is deducible by means of T from the G-characterisation of the D-parts of w. (We construe ' A is deducible from B by means of C ' to exclude the cases in which A is deducible from B alone or from C alone.)

A (suitably relativised) derivable attribute is one which may appropriately be said to be *explained* by T. (Such an attribute of an object is sometimes also said to be *reduced* to certain attributes [i.e. those in G] of its parts.) An example from chemistry: Let w be a molecule whose parts (D) are four different atoms (or radicals) attached to one carbon atom (called ' asymmetric ' in this case) ; let G be the chemical and physical properties of these parts, and T be the laws of stereochemistry. Then the molecule will have the derivable property of being *optically active*, i.e. rotating the plane of vibration of polarised light, since this follows by T from the asymmetry of the carbon atom.

Special interest attaches to the concept of attributes of a whole underivable from information regarding its parts even by using the accepted laws and theories of the sciences.

In his pioneering study, ' Über Gestaltqualitäten ',[1] Christian von Ehrenfels first directed the attention of psychologists to instances of perceptual properties of objects which are characteristic attributes of wholes in the sense considered in this section—the so-called *Gestaltqualitäten*. It is to this end that the first condition for wholes given by von Ehrenfels is directed—the so-called *First Ehrenfelscriterion*, i.e. the condition that : ' The whole is more than the sum of its parts.' [2]

And indeed the vagueness introduced by the phrase ' any possible ' deprives even the belief of any clearly statable content. (See C. G. Hempel's ' General System Theory and the Unity of Science ', *Human Biology*, 1951, **23**, 313-322.)

Again, our discussion has connections with the holistic position in the philosophy of biology. (See J. C. Smuts, *Holism and Evolution*, New York, MacMillan, 1926.) Our concept of an underived attribute of a whole could, in this connection, be taken as a precise explication of the informal concept of a *holistic property*. However, to avoid possible metaphysical connotations we do not use the term ' holistic '.

[1] *Vierteljahrschrift für wissenschaftliche Philosophie*, 1890, **14**, 249-292

[2] For a critical analysis of this condition the reader is referred to E. Nagel's ' Wholes, Sums, and Organic Unities ', *Philosophical Studies*, 1952, **3**, 17-32. In fact, it is obvious that its validity depends upon a specification of the meaning of the key terms, ' whole ' ' more ', ' sum ', and ' part ' ; and the statement of the criterion employs none of these terms in its accustomed sense.

3 *Dependence Systems*

We turn now to item (ii) in our list of conditions for wholes, the dependence of certain characteristics of one part upon those of other parts. The following example will illustrate this idea.

In an illustrative device of A. Meyer, magnetic needles of equal strength are inserted in pieces of cork, and floated, with all like poles upwards, in a basin of water. A strong magnet of unlike pole is placed in a fixed position overhead. The floating magnets arrange or rearrange themselves in a symmetric pattern of one or more concentric circles, depending upon the number of magnets.[1] In this type of configuration there exist various dependencies ; for example, the distance of one cork from the nearest adjacent cork depends upon the magnetic strengths, and the number of other magnets.

In a general characterisation of the concept of dependence here exemplified we will have to refer to a set of objects which, by virtue of standing in certain specified relations, are said to form a configurational whole, or briefly, a configuration. A particular kind of configuration may then be characterised by specifying the various relations in which its constituents, or ' parts ', must stand ; those relations jointly may always be viewed as constituting one more complex relation, R. An ordered set of objects, $p_1, p_2, \ldots p_n$, which stand in the relation R to each other, i.e. for which $R(p_1, p_2, \ldots, p_n)$ holds, will be said to form a *configuration of kind R*. In many cases of interest for empirical science, the attributes among which a dependence relation obtains are quantitative in character, i.e. they are attributes with numerical values, such as distance from a fixed reference point, temperature, charge density, and the like. We will therefore formulate our general characterisation of dependence for this quantitative case, an extension to non-quantitative properties and relations being readily possible. Dependence in this sense will consist in a (more or less complex) relationship ϕ—in many cases a functional relationship —between the value of the dependent attribute for p_1, and the values of certain attributes for the parts of the configuration. We thus arrive at the following definition :

Definition 4

A quantitative attribute f of the part p_1 in a configuration of kind R consisting of the n objects $p_1, p_2, \ldots p_n$ is ϕ-dependent

[1] D. W. Thompson, *On Growth and Form*, Cambridge University Press, 1942, 315-316

upon the class G of quantitative attributes of these objects p_i, if ϕ is a relationship such that in *every* configuration of kind R the f-value of the first member of this configuration is related by ϕ to the values which the quantitative attributes in G assume for the parts $p_1, p_2, \ldots, p_n$.[1]

Thus to assert a dependence relation of this type is to assert a *law-like connection.*

The concept of dependence as defined here can be generalised so as to be taken as a matter of degree, which varies with the extent to which ϕ determines or delimits $f(p_1)$ in terms of the $g_i(p_j)$, where g_i is the i^{th} attribute belonging to the set G. If ϕ is ' strong ' it will determine $f(p)$ completely if the $g_i(p_j)$ are known; as when classical economic theory specifies the price (f) of a commodity (p_1) in terms of the supply and demand (g_1, g_2) of the ingredients (including labour) of its make-up $(p_2, p_3, \ldots, p_n)$. On the other hand, ϕ may be ' weak ' when (as in the case of a statistical rule) it provides only probable information regarding $f(p_i)$ if the $g_i(p_j)$ are known. It is one of the major tasks of statistics to devise measures of the extent to which one set of values determines another, associated value.

In an important special case of dependence, the configuration consists of just one object, p. Then the value f for p is ϕ-dependent upon the values of the quantative attributes $g_1, g_2, \ldots, g_n$ for p.[2] It is in this sense that the weight of an animal of a given species is ' *roughly* dependent ', relative to some statistical relationship ϕ, upon its age and sex.[3] Another instance of ϕ-dependence in this sense is furnished by Van der Waals' law, which determines the pressure of a body of gas as a function of its volume and temperature.

[1] Symbolically

$$(x_1)(x_2)\ldots(x_n)[R(x_1, x_2\ldots, x_n) \supset \phi(f(x_1), g_1)(x_1), \ldots, g_1(x_n),$$
$$g_2(x_1), \ldots, g_2(x_n), g_3(x_1), \ldots, g_3(x_n), \ldots, g_k(x_1), \ldots, g_k(x_n))],$$

where $g_1, g_2, \ldots, g_k$ are the numerical attributes included in G.

[2] That is, $(x)\phi(f(x), g_1(x), g_2(x), \ldots, g_n(x))$ obtains.

[3] The methodological importance of such ' rough dependencies ' among related quantities for the social sciences is stressed by H. D. Lasswell and A. Kaplan in *Power and Society*, New Haven, Yale University Press, 1950, 29-32 *et passim.* Indeed their definitions of various basic sociological concepts are framed along such lines.

Of course, one of the main tasks of the investigation is to decrease the element of ' roughness ' in known dependencies. This point is well illustrated in K. J. Arrow's interesting article ' Mathematical Models in the Social Sciences ', *The Policy Sciences,* ed. by D. Lener and H. D. Lasswell, Stanford, 1951, pp. 129-54.

A second special case of dependence is that in which the set G consists of the single attribute f itself. In this case, the f-value for p_1 is connected, by ϕ, with the f-values for $p_2, p_3, \ldots, p_n$. Electrostatics is the source of the classical example of this concept, employed by Köhler. The charge-density at any point of a given charged, well-insulated electric conductor is ϕ-dependent upon the charge-densities at the remaining points, relative to a complex condition specifiable in electrostatics. Biological homeostasis also provides an illustration. For example, body temperature is constant in changing environments ; and the content of individual ions in one volume of the fluid of an animal is very nearly the same as the ion-content of other, equal volumes.

As mentioned before, an analogous concept of dependence can be defined for non-quantitative attributes. Without entering here into the details of the definition, we will simply mention two examples of this kind of dependence.

Consider a configuration of coloured areas. The perceived colour of a given area p_1 in the configuration depends upon the actual colours of all the constituent areas by virtue of the laws of biophysics and the physiology of colour perception. Again, in the Müller-Lyer illusion, line segments of equal length appear unequal when additional lines are drawn, two pairs making acute angles with the ends of one segment, and two pairs making obtuse angles with the ends of the other. Such phenomena show that the perceived character of a part of a structure may depend upon the character of the neighbouring parts, relative to conditions the study of which is a central object of investigation in Gestalt psychology.

Other aspects of the concept of dependence have been studied by K. Grelling and P. Oppenheim,[1] with a special view to their bearing on the Gestalt concepts. One of the main conclusions of this analysis is that one of the principal senses in which the term *Gestalt* has commonly

[1] K. Grelling and P. Oppenheim, ' Der Gestaltbegriff im Lichte der neuen Logik ', *Erkenntnis*, 1938, **7**, 211-225. ' Logical Analysis of " Gestalt " as " Functional Whole " ', by the same authors ; preprint, distributed at the fifth International Congress for the Unity of Science, Cambridge, Massachusetts, 1939. K. Grelling, ' A Logical Theory of Dependence ', preprint, similarly distributed (vol. 9 of the *Journal of Unified Science*, in which these papers were to appear, was never published, due to war conditions). In these papers aspects of dependence, and of several related concepts, such as independence and interdependence, are studied. Much of the credit in connection with these clarifications is due to Grelling, who fell a casualty to the Nazi terror.

been employed is to characterise wholes whose parts have properties, which are, in effect, ϕ-dependent attributes (relative to a relationship ϕ derived from a universal law, and to some of their scientifically interesting properties G). (The familiar insulated conductor with its equilibrated surface-distribution of electricity is the best-known example.) Such wholes Grelling and Oppenheim term *dependence systems*. These considerations underlie the definition :

Definition 5

A configuration is a *ϕ-dependence system* relative to a set G of attributes if each part of the configuration has some G-attribute which is ϕ-dependent upon (some or all of) the G-attributes of (some or all of) the remaining parts.

The conception of biological organisms as dependence systems seems to be adumbrated by Georges Cuvier's statement :

> Every organized being forms a whole, a unique and perfect system, the parts of which mutually correspond, and concur in the same definite action by a reciprocal reaction. None of these parts can change without the whole changing ; and consequently each of them, separately considered, points out and marks all the others.[1]

The concept of dependence system furnishes a logical reconstruction of the informal conception of an ' organic ' or ' functional ' or ' integrated ' whole. For this is based upon the intuitive requirement that the parts of a whole must stand in some special and characteristic relation of interdependence with one another, in virtue of their status as parts of the whole. As the examples we have discussed indicate, what is intended here can be expressed with increased precision by use of the schematism of the dependence-system concept.

> What we have termed a ' dependence system ' provides a precisely defined counterpart of what has been described as systems ' the behaviour of which is not determined by that of their individual elements, but where the part-processes are themselves determined by the intrinsic

[1] *Discourse on the Revolutions of the Surface of the Globe*, Philadelphia, 1831. From the French (*Discurs sur les révolutions de la surface du globe*). And indeed the concept of biological organisms as systems integrated by specific patterns of interration ships has a notable history from the time of Goethe and Cuvier to the present day. In this connection, some interesting contemporary ideas are discussed in A. B. Novikoff's paper, ' The Concept of Integrative Levels and Biology ', *Science*, 1945, **101**, 201-215.

Interdependencies of this kind have formed a model for investigations in other fields, for example, in the science of *linguistic* structure. (See E. Cassirer, ' Structuralism in Modern Linguistics ', *Word*, 1945, **1**, 99-120.)

nature of the whole '.[1] Any dependence system furnishes an example of a whole whose parts have features which are such that knowledge of them cannot, relative to the available theoretical information, be acquired by studying their parts in isolation, but requires information regarding other parts. In consequence, the concept of a dependence system also provides a natural housing for the organismic biologists' claim that ' analysis of living processes into the behaviors of distinguishable parts of organisms entails a radical distortion of our understanding of such processes '.[2] The study of the logic of such dependence systems would, thus, be an integral part of a General System Theory in the sense of von Bertalanffy.[3]

Configurations of the kind here considered are, of course, of special interest when analysed also from the point of view developed in the preceding section. The system of floating magnets, for example, has the *derivable* attribute of central symmetry, deducible from the specified characteristics of the constituents by virtue of the relevant physical laws. In other cases, similar attributes may *not* be derivable by means of the accepted laws and theories of empirical science. Thus the legs of insects (hexapodes) in motion form a configuration in which the timing of movement for each leg is functionally dependent upon that of the others. When some of the legs are amputated a similar relation obtains among the remaining legs,[4] just as after removal of some of the floating magnets the remaining ones again arrange themselves in a symmetrical pattern. In the present state of our knowledge, this characteristic rhythm of the whole sequence of leg movements is an *underivable* attribute in the sense of not being deducible with the help of empirical laws from the physiological attributes of the parts of the insect.

4 *Structural Features of Wholes*

We now pass on to item (iii) in the list of conditions for wholes : that the whole must possess some kind of structure, in virtue of which

[1] M. Werthemier, *Über Gestalttheorie*, Erlangen, 1925, p. 43

[2] This version of the claim is given by E. Nagel on p. 339 of ' Mechanistic Explanation and Organismic Biology ', *Philosophy and Phenomenological Research*, 1951, **11**, 327-338.

[3] A good expository account is von Bertalanffy's article ' Problems of General System Theory ', *Human Biology*, 1951, **23**, 302-312. For further details the reader is referred to the references cited there. Some suggestive criticisms are given by C. G. Hempel, ' General System Theory and the Unity of Science ', ibid. 313-322.

[4] See *Handbuch der normalen und pathologischen Physiologie*, Berlin, Verlag von Julius Springer, 1931, **15**, Zweite Häfte, 1076

certain specifically structural characteristics pertain to it. These structural features of wholes are of interest because one important idea covered by the term 'whole' is that of a structured organisation of elements. A structured whole in this sense involves three things: (1) its *parts*, (2) a *domain of 'positions'* which these parts 'occupy' (this need not necessarily be spatial or temporal, but may have any kind of topological structure whatever), and (3) an *assignment* specifying which part occupies each of the positions of the domain. A (particular) performance of a musical composition is the classical example of a whole in this sense: the time-interval of the performance serves as the *domain of positions*, the various tones (as characterised by their pitch, volume, and duration) which are played throughout the performance are the *parts*, and the score which fixes the distribution of these notes throughout the performance is the *assignment*.

Now, in scientific discussion, we often do not know (or care) about the *individual* parts which occupy the various positions. An example from chemistry: when discussing the molecule H-O-H, it suffices that some O atom should occupy the place corresponding to the centre space of the representation; it makes no difference in our discussion which particular oxygen atom this is. These considerations lead us to the concept of a *complex*. We will say that whenever any structured whole is considered with a view to the types of its parts, rather than its specific parts themselves, it is viewed as a *complex*.[1]

[1] Definition 6:

A *complex* is characterised by the following three features:

(1) A set G of topologically structured attributes.

(2) A topologically structured space X, constituting the domain of positions.

(3) An assignment f of exactly one G-attribute to each X-position.

Many sets of attributes have a natural topological structure, as in the case of the quantitive attributes, or the well-known three-dimensional discrete ordering of the colours. We will assume throughout this section that attributes dealt with are topologically structured. The attributes constituting the set G may be multiple attributes in the sense of being ordered n-tuples of attributes. Thus an attribute of a sound (tone) is the combination of pitch, volume, and duration.

An example of a domain of positions X is a time interval, with the usual topology of the continuum. The space X may have any topological structure whatever; such 'spaces' as the *Bauplan* of architecture or biology being definitely included.

This definition reproduces, in a somewhat generalised form, the definition of 'complex' given in the paper 'Der Gestaltbegriff im Lichte der neuen Logik' by K. Grelling and P. Oppenheim, *Erkenntnis*, 1938, 7, 211-225. J. von Kempski, in an interesting and suggestive article, first proposed a generalisation of that definition,

A temperature-chart for an ill person is an example of a complex: G, the *set of attributes* is the set of temperature-values, X, the *domain of positions*, is the time-interval under consideration, and f, the *assignment*, is the graph assigning the patient's temperature throughout the time-interval.

> For the purposes of some disciplines, such as chemistry, it is of interest to distinguish two kinds of 'decomposition' of a complex, each of which yields one of the two principal constituents: the set G of component-types, and the topologically structured space X. For example, if for a given molecule—say that of benzene—the structural arrangement of the constituent atoms is disregarded, there remains the 'molecular formula', C_6H_6, which makes it indistinguishable from another quite dissimilar compound. If, on the other hand, the types of the atoms are disregarded, there remains the 'structural formula', the hexagonal ring, which has in itself interesting chemical attributes and may be occupied by other atoms than carbon.

We must now examine the conditions under which complexes have the same 'structure', that is to say, are *isomorphic*. There is, first, the most obviously structural element of the complex, its domain of positions X. We impose on *isomorphic* complexes the condition that their domains of positions must have the same topological structure, i.e. be *homeomorphic* in the topologists' sense. Next, we must require that the sets of attributes of the two complexes, G_1 and G_2, should have the same structure. That is, there must exist a one-to-one correspondence (pairing) F of G_1 and G_2 which preserves the type[1], and the topological properties of these two sets. Finally, we must require that the classifying assignments, f_1 and f_2, of the two complexes should assign G-attributes paired by F to corresponding X-positions.[2]

which he terms 'typifikator' (*Studium Generale*, 1952, **5**, 205-218). The present definition is so framed as to comprehend that of von Kempski. (The typifactor is the special case in which the domain of positions X is a *lattice* in the sense of mathematical lattice-theory.)

[1] Attributes are of the same 'type', as here used, if they represent the same general type or category of thing (e.g. colours, ages, shapes). The purpose of the requirement that the G_i must have the same topological structure will appear more clearly in connection with the applications discussed below. (The variations of a theme need not have the same melody, nor a Bach fugue and its 'mirror fugue'.)

[2] We thus arrive at the definition:

Definition 7:

Two complexes (X_1, G_1, f_1) and (X_2, G_2, f_2) are *isomorphic* if

(i) X_1 and X_2 have the same topological structure,

For an example let us take one musical composition, and a second one obtained from the first by a prescribed change in its pitch alone, a 'transposition' (in its musical sense). The original composition is a complex (X_1, G_1, f_1) assigning (f_1) to each moment of a time-interval (X_1) a certain tone (G_1). The second, transposed, composition is similarly a complex. The mode of musical transposition establishes the one-to-one correspondence between G_1 and G_2; it provides the function F which (in leaving unchanged the volume and duration, but providing a specific modification in the pitches of the tones) assigns corresponding G_i-types to corresponding X_i-positions (times).

Again, temperature charts which have the same shape are instances of isomorphic complexes. Each temperature graph assigns (f_i) a certain numerical temperature value (G_i) throughout equal time intervals (X_i). The graphs have the same shape (the same slope at each abscissa), and so the corresponding f_i-values will differ by at most a (fixed) constant, which fact yields the required function F.[1]

Our definition of *isomorphism* of complexes has been so framed as to accommodate so far as possible the intuitive conception of identity of structure, as applicable to complexes. It is therefore to be expected that complexes which are isomorphic in our sense will, in the

(ii) there exists a one-to-one correspondence F of G_1 and G_2 which preserves their type, and all of their topological properties, and

(iii) the assignments f_i assign F-corresponding G_i-attributes to corresponding X_i positions.

This definition of the isomorphism of complexes is closely analogous to that of sameness of structure of class-ordering relations. See B. Russell, *Human Knowledge*, New York, MacMillan, 1948, 254, and see also J. H. Woodger's contribution 'On Biological Transformations' to *Essays on Growth and Form Presented to D'Arcy Wentworth Thompson*, Oxford, Clarendon Press, 1945, 98–99. (Woodger utilises the definition in connection with a definition of the biological concepts of *homology* and *Bauplan*.)

[1] It is of interest to remark that in some important special cases it is possible to extend this qualitative (yes or no) notion of isomorphism or sameness of structure to a quantitative measurement of similarity of structure. If both f_i assign to the X_i positions places in a discrete linear ordering L (as will happen, for example, when the G_i are a finite set of temperature values), then the degree of structural similarity of (X_1, L, f_1) and (X_2, L, f_2) can be measured by means of statistical rank-correlations. In the subcase that L consists of points on the real number axis (i.e., the f_i are real-value functions), the degree of structural similarity of the complexes can be measured by means of coefficients of correlation.

The substance of this remark is due to Dr H. E. Hartley of the University of Miami, who was kind enough to send the authors his typescript on 'Correlation Coefficients and Gestalten'.

application of our formalism, possess structural similarities. And indeed this expectation is readily verified. In the case of our musical example, the isomorphic complexes—transposed compositions—have a significant common structural characteristic, they have the same *melody*. Again, the isomorphic temperature charts have the same *shape*. Thus, isomorphic complexes, it appears, generally share some basic characteristic of a structural nature : e.g. the same *melody* in the case of a musical composition, or the same *shape* in the case of a temperature chart. Complexial isomorphism provides a basis for the *classification of complexes* with respect to such structural characteristics as melody, shape, pattern (of arrangements), meter (of poetry), and the like.

It is illuminating to consider the concept of isomorphism of complexes in somewhat greater detail. Isomorphic complexes are related by certain *transformations* (in the mathematical sense). In fact, there exists a family of specifically related modifications—*a group of transpositions*—linking all complexes which are isomorphic with one another. It is thus always possible to effect the transition from one complex to another isomorphic with it by means of some transformation belonging to this family (i.e. the *group*).[1]

In illustration of these ideas, let us consider what are called ' transpositions ' in musical theory. The pitch of the members of a sequence of tones can be varied systematically in many ways. Here the group of all abstractly possible transpositions has the subgroup of the transpositions of music. For if we compare two played sequences of tones the second of which has been obtained from the first by, say, transposition of an octave, their tones are temporally correlated in such a way that a certain specific and strictly definable relation obtains

[1] The transpositions of this group are definable in terms of the correspondences that obtain between isomorphic complexes. As defined above, two complexes (X_1, G_1, f_1) and (X_2, G_2, f_2) are isomorphic if there is a topological homeomorphism H of X_1 and X_2, and a topology-preserving 1-1 mapping F of G_1 and G_2, such that the f_i assign F-corresponding G_i-attributes to H-corresponding X_i-positions. Thus isomorphism of complexes requires the existence of two mappings, an homeomorphism $H : X_1 \leftrightarrow X_2$, and a topology-preserving 1-1 correspondence $F : G_1 \leftrightarrow G_2$, which together satisfy the additional requirement that for any element (position) x of X_1, $F(f_1(x)) = f_2(H(x))$; or abstractly, that $f_2 = Ff_1oH^{-1}$, where o is function composition, and H^{-1} is the inverse of H. If we term such an f_2 a *transposition* of f_1, it is readily seen that the set of all transpositions constitute a group (in the algebraic sense), with respect to function composition as group operator. This group can be conceived of in either of two equivalent ways : as a group of transformations (i.e. the transpositions) or as the equivalence-set of the corresponding equivalence-relation (i.e. transponibility).

between the pitches of corresponding tones (namely, the sequence of intervals). Thus if a musical composition is viewed as a complex, a musical transposition is a transposition in our sense.

In this musical application we see also that important structural characteristics of a complex may in the applications be associated not with the entire group of transpositions, but with some particular subgroup. The musical composition (complex) has an attribute (is a melody) which is unchanged or *invariant* under transpositions belonging to a subgroup of the group of all abstractly possible transpositions (the transpositions in the sense of music). These considerations underlie the definition :

Definition 8

Given some subgroup Z of the group of all transpositions of a complex, we designate as a *complexial feature* (relative to Z) of this complex any attribute which it shares with all complexes differing from it only by Z-transpositions, i.e. any attribute *invariant* under these transpositions.

Thus, for example, in the case of such a complex as a grouping of black spots on a white background, the phenomenal grouping (seen pattern) of the ensemble is a complexial feature relative to the group Z of certain changes of distance (the contractions and dilatations) which leave invariant the attribute in question.[1]

In the applications of this schematism of *complex* and *isomorphism*, it may happen that a complexial feature reflects an attribute of a complex which is *underivable* in the sense of our earlier discussion (Definition 3 . 1), and this, consequently, is of special interest. Also, a given complexial feature (say a visual pattern which involves an optical illusion) may reflect a dependence (the optical illusion ; cf. p. 97). And similarly, the charge distribution on an insulated conductor may be analysed either as a dependence system, or as a complex. Thus we see again that several of the concepts we have considered may, in the applications, fuse in their bearing upon one and the same object of investigation.

It should be remarked that the possession of such attributes, which are invariant under transpositions, is another condition for *Gestalten*, the so-called *Second Ehrenfels-criterion* (cf. p. 94 above).[2]

[1] M. Wertheimer has investigated in a classical study the laws of organisation governing the perception of dot-formations. (' Untersuchungen zur Lehre von der Gestalt, II ', *Psychologische Forschungen*, 1923, 4, 301-350.)

[2] The purport of this requirement has been well put by W. Köhler : ' it is

An important application of these concepts is to cases in which something is *constant* under varying processes or modifications.[1] For if the object in question can be represented as a complex in our sense, and its modifications as transpositions, the constancy will amount to a complexial feature. Significant instances in the applications are furnished by physical systems that attain a state which is independent of time, a stationary state or state of equilibrium : a state left invariant by the processes of nature.[2] Such systems are characterised by the fact that they satisfy some extremal condition or extremal principle on the physical quantities involved (e.g. energy a minimum, entropy a maximum, or the like).[3] These systems have played a significant rôle in Gestaltist discussions, and have led to the notion of a ' good Gestalt '. An object is considered a good Gestalt in the degree to which it exhibits invariance or stability,[4] i.e. an object which is called a Gestalt relative to some particular structural characteristic is a good

characteristic of phenomenal Gestalten that they may retain their specific properties even when the absolute constituents upon which they rest are varied in certain ways '. (*Die physischen Gestalten in Ruhe und im stationären Zustand*, Erlangen, 1920, p. 37. Portions of this work are translated by W. D. Ellis in his *Source Book of Gestalt Psychology*, London, Routlege and Kegan Paul, 1938.) An illuminating discussion of perceptual transposition is found on pp. 196-205 of Köhler's *Gestalt Psychology*, New York, Liveright, 1947. It may be noted, however, that the concept of transposition (and thus the Second Ehrenfels criterion) is not limited in applicability to spatio-temporal transformations.

The term ' Gestalt ' as commonly used is quite general, and applies not only in the case of the Gestalt-qualities as discussed here, but also designates dependence systems and complexes. It has been used throughout this paper in such a generic sense.

[1] It is of interest to remark in this connection that chemistry may be defined as that science which deals with transformations of the outer electronic shell of the elements that leave their atomic number invariant.

Of course, constancy under modifying processes is not confined to complexes, and the tendency of a ' whole ' or ' system ' to maintain its ' structure ' may apply also, e.g. in the case of dependence systems. Biological homeostasis is a case in point, and equilibrium analysis in economics can provide other examples. K. Deutsch has spoken of *social* homeostasis (' Mechanism, Teleology and Mind ', *Philosophy and Phenomenological Research*, 1951, **12**, 185-223). (Cf. also H. D. Lasswell and A. Kaplan's *Power and Society*, New Haven, Yale University Press, 1950 pp. xv-xvi.)

[2] The significance of such systems for the theory of Gestalt was first studied by W. Köhler in *Die physischen Gestalten* . . . , Erlangen, 1920.

[3] A detailed discussion may be found in K. Koffa's *Principles of Gestalt Psychology*, London, Kegan Paul, 1936, pp. 174 ff.

[4] An instance of a poor perceptual Gestalt is the picture giving a line-representation of a staircase, which shifts indecisively between being upright and inverted.

Gestalt to the extent in which this characteristic is unaffected by various processes or modifications.

Gestalt theorists have been concerned with extremal properties precisely because these are indicators of stability, i.e. of invariances under transposition. Wertheimer's *Prägnanzgesetz* is a statement to the effect that in perception Gestalten tend to become as 'good' (i.e. stable or stationary or invariant under the relevant transpositions) as the boundary value conditions, the physical basis of the perception, will permit. Thus in the case of the Gestalten of visual perception, a premium is put on symmetry, simplicity of linear structure, and uniformity of component structures, since all of these lead to invariances under certain groups of geometric transpositions.[1]

5 *Summary*

We have distinguished in the course of our investigations three basic types of concepts which are relevant for the characterisation of various kinds of 'wholes' and Gestalt phenomena: (1) the derivable and the underivable attributes of wholes, (2) the dependent attributes of parts within wholes, and (3) the structural features of wholes. As we have seen, these types of Gestalt concepts, although logically distinct, often have applications to one and the same object of scientific enquiry.

It is hoped that the logical analysis here presented will facilitate a re-assessment of the significance of Gestalt concepts for empirical science.[2]

Such perceptual structures which require stabilisation or even completion on the part of the observer have been much studied by Gestalt psychologists.

[1] For a mathematical treatment of these matters see the discussion of the symmetry of ornaments in A. Speiser's *Theorie der Gruppen von endlicher Ordnung*, Berlin, Springer 1927, and also G. D. Birkhoff's *Aesthetic Measure*, Cambridge, Harvard University Press, 1933, and H. Weyl's *Symmetry*, Princeton, Princeton University Press, 1951.

[2] Current opinion as to the importance and scope of Gestalt concepts in scientific enquiry is divided. Some scholars, e.g. E. H. Madden, hold that these concepts are unnecessary, and the basic Gestalt theses unwarranted. ('The Philosophy of Science in Gestalt Theory', *Philosophy of Science*, 1952, **19**, 228-238.) For a brief critique of Madden's contentions, see N. Rescher's 'Mr Madden on Gestalt Theory', *Philosophy of Science*, 1953, **20**, 327-328. Still others wish to restrict the Gestalt concepts to psychology. A clear conception of the large scope which can, however, be claimed for these concepts may be obtained from the September, 1952, number of *Studium Generale*, which contains a symposium on the concepts of *whole* and *Gestalt*. To indicate one, perhaps surprising, relevancy to the theory of value, we refer the reader to C. I. Lewis' *Analysis of Knowledge and Valuation*, La Salle, 1946, especially pp. 486-488.

ONE-WAY PROCESSES IN PHYSICS AND BIOPHYSICS *

L. L. Whyte

The philosophy of science covers many subjects and employs a variety of methods. I wish to call attention to a task falling within its scope to which I consider that more attention might be paid : the discovery of directions in which scientific theory can profitably advance by the identification of experimental, mathematical, or theoretical problems which lead into new regions of enquiry. For example, the philosophy of physics, pursued in an appropriate manner, can suggest lines along which physical theory might develop and formulate problems for the experimenter and mathematician.

Philosophical considerations have certainly eased the advance of science in the past ; indeed philosophical speculation has often anticipated the path that was later followed by exact science. In certain periods disciplined speculation is as necessary as experiment, mathematics, and systematic reasoning. This will perhaps be agreed. But it has not yet been sufficiently recognised that scientific speculation is an important branch of the new discipline of the philosophy of science. Indeed, it would be a dark day for science when only papers reporting positive results on special problems were considered worthy of publication.

This paper has therefore a dual purpose : to use philosophical analysis to identify physical problems to which greater attention might be paid, and thereby to illustrate a legitimate activity in the philosophy of physics and biophysics.

Major advances in physical theory consist in the discovery of more general laws which include the established ones as special cases. The pause in fundamental theoretical physics since 1925–28 may therefore

* Based on the Chairman's Address to the Philosophy of Science Group, 11th October 1954. This paper is complementary to last year's Address (this *Journal* 1954, 5, 1), in which the following correction should be made : on p. 15 the large bracket should be replaced by :

$$\left\{1 + \frac{\alpha^2}{(n' + \sqrt{k^2 - \alpha^2})^2}\right\}^{-\frac{1}{2}}$$

imply that some obstacle lies in the way of the discovery of the generalisation of quantum mechanics which can provide a general theory of particles. A generation devoted to the collection of new facts and the improvement of mathematical techniques has failed to produce methods of adequate power. What is the obstacle? I suggest that a new principle may be required to which no direct transition is possible within the assumptions common to the theories of this century. To broaden the basis of physics sufficiently to cover high energy properties it may be necessary to reconsider the presuppositions both of relativity theory and of quantum theory and to pass beyond the range of any correspondence principle of the usual kind. To reach the new *terra firma* we may for the first time have to pass out of direct sight of the classical shores.

This does not mean that we are reduced to waiting, passively, for a new insight. Major theoretical advances have often been preceded by a period of tentative exploration. During such phases of hesitant preparation, unsuccessful investigations can be of value in easing the subsequent advance, provided that they lie in the correct direction.

The lesson is clear : *it is now time to undertake a systematic study of the possible generalisations of current methods.* It may be that the required idea cannot appear until *after* an extended systematic exploration which has scarcely begun. The consideration of non-linear equations, *S*-matrices, and non-localisable fields may be only the first steps in an investigation which will not yield results until more radical methods are employed.

The present paper is part of a continuing study of a possible generalisation of quantum mechanics. The aim is to focus attention on definite problems, experimental or mathematical, which have so far been relatively neglected ; possibly because they may lie in the realm of the next physical theory rather than that of existing theories. This new realm is *the physics of complex changing structures* and the need is for a theory of the simple aspects of their complex processes.

Before 1900 fundamental physics was concerned with problems involving very few variables, with often only two centres of force, though statistical mechanics was treating the simpler aspects of systems of theoretically infinite complexity. Quantum mechanics in principle covers all atomic-molecular stationary states, and all low energy processes. But the treatment of even the simpler chemical reactions involves awkward approximative methods and no elegant method is available for describing the changes in systems of finite complexity.

What is required is a physics of changing ordered complexity, covering the changes of systems more complex than a sun and a planet, and more ordered than an ideal gas. May not complex structural processes have simple aspects which can be represented, for example, by one variable?

The advance from quantum mechanics towards this more comprehensive theory may be investigated from three points of view, which place the emphasis respectively on the spatial relations, the temporal relations, and the number of particles or variables involved. My Address to the Group last year dealt primarily with the spatial aspect, and suggested that the expressions now in use to represent quantitative spatial relations, and magnitudes derived from them, could be generalised by using dimensionless variables of a particular form.[1] The present paper takes up the second aspect : the temporal relations, and asks : *how can the temporal relations assumed in the basic expressions of classical, relativity, and quantum mechanics be generalised in a more comprehensive theory?*

The temporal relations of physics display various aspects, of which the most important are the quantitative measure of time provided locally by clocks ; the extension of this local time co-ordinate through space ; and the asymmetrical relation of succession, which vanishes in certain idealised processes representing reversible limiting cases. Here we shall neglect the first two, which are closely linked with the spatial relations treated last year, and concentrate on the relation of succession.

In this respect a definite choice confronts physical theory, a choice which can no longer be left unexamined in view of the need to generalise present methods :

(*A*) A basic theory founded on *symmetrical relations*, and equations used to represent fundamental laws governing reversible processes and stationary states with constant energy. The asymetrical relations required to represent irreversible processes then possess an anomalous status and have to be introduced as obscure consequences of interactions or statistical effects, perhaps ultimately ascribable to boundary conditions which involve the relation of succession. This covers not only the existing state of physical theory,[2] but also most of the tentative generalisations that have already been proposed.

[1] i.e., of the form $(l_0/l,)$ (t_0/t), (m_0/m) in an obvious notation. See this *Journal*, 1951, **45**, 1.

[2] Though the general equations of quantum mechanics are of reversible form,

(*B*) A basic theory founded on *asymmetrical relations*, and inequalities used to represent a very general class of one-way processes, as in entropy theory but with greater scope. This implies that the new fundamental concepts are to be reached by transforming the concept of entropy, which is restricted to the stationary states of systems in which there is local equilibrium (or small deviations from such states), to cover processes involving the structured disequilibria of atoms and molecules. In a theory of this type the symmetrical temporal relations and equations of reversible physics are no longer treated as fundamental, but have to be derived as limiting cases which arise only under special conditions, cases in which the one-way aspect can for certain purposes be neglected.

The alternative (*B*) is the one now to be explored. It suffers from serious disadvantages: it is strange; it involves a reconsideration of much of physical theory; and it challenges the current view that the present difficulties of particle theory should be overcome without introducing temporal asymmetry explicitly into the basic laws.

But the disadvantages and dangers of rejecting (*B*) are even graver, for then physical theory is permanently doomed to extensive regions of obscurity and ambiguity. One cannot reason elegantly and reliably from narrower premisses to more general conclusions, since the more general relations have then to be introduced surreptitiously by some obscure procedure which is not what it pretends to be. A comprehensive physics must certainly make extensive use of the asymmetrical temporal relation of succession. Therefore, if elegance and lucidity are desired, it is appropriate to try postulating new basic principles of the more general type (*B*), and to investigate whether it is possible to reason back from them to the narrower principles already established. We should give up the long struggle with the question: 'How does irreversibility arise if the basic laws are reversible?', and ask instead: 'If the laws are of a one-way character, under what spatial and temporal, quantitative and steric conditions can reversible expressions provide a useful approximation?'

The distinction between theories of type (*A*) and of type (*B*) is not merely one of differing degrees of formal generality, for type (*B*) is also more powerful physically, since it can in principle achieve

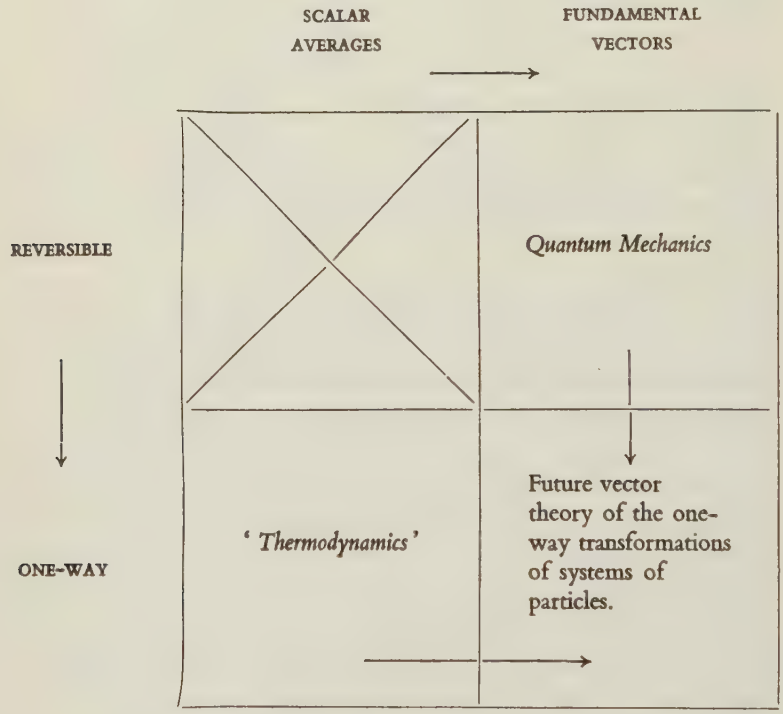
solutions involving external axial vectors (rotations, magnetic fields, spins) are not reversible unless these vectors are also reversed. Thus, no electronic motion is strictly reversible unless all other electronic motions and spins are also reversed.

something that is beyond the scope of theories of type (*A*) : the determination of the quantitative and other conditions under which reversible expressions are valid. Moreover, an urgent task in the philosophy of exact science is the elucidation of the general relationship of cyclic to one-way processes. This calls for the broader basis of a theory of type (*B*).

The significance of a transformation of fundamental theory to type (*B*) can be clarified by noting that while quantum mechanics employs reversible equations to represent the relations of fundamental vectors, and thermodynamics one-way inequalities to represent differences in scalar averages, the new theory would employ one-way inequalities to represent changes in fundamental vectors. Thus the next clarification would involve *two* simultaneous and convergent advances : from *reversible vector theory* (quantum mechanics) and from *one-way scalar theory* (thermodynamics) towards a *one-way vector theory* covering problems outside the scope of either of its predecessors. This shows that the task is not merely to generalise quantum mechanics, but in doing so simultaneously to generalise the current thermostatics, misleadingly called thermodynamics. An extension of the Onsager-Prigogine-de Groot theory of the steady states of irreversible processes to cover the presence of local molecular-atomic disequilibria and provide a theory of the detailed (? discontinuous) structure of entropy production may prove equivalent to a transformation of quantum theory introducing the concept of one-way process into its basic postulates. This dual advance is symbolised in the diagram where arrows represent steps towards expressions of higher generality. The aim is a theory based on a one-way process which can be regarded as the expression either of a quantised entropy principle or of an irreversible generalisation of quantum laws. This amounts to the suggestion that particle theory can benefit by taking some hints from recent thermodynamics.

In the 1950s this is a reasonable aim. From 1600 to 1824 physics neglected irreversible processes. From 1824 (Carnot) to 1954 irreversible processes have been increasingly studied, but the fundamental laws have still been assumed to be reversible, the irreversible aspects of actual processes being ascribed to secondary causes (statistical effects, interactions, measurement, boundary conditions). The time would appear to be ripe for exploring the consequences of treating the logically more general type of process as also theoretically fundamental.

The terms ' one-way ' and ' irreversible ' have so far been used indiscriminately to refer to any process involving the asymmetrical relation of succession. ' Irreversible ' has many meanings, some of which are vague and misleading. Many so-called ' irreversible processes ' can under certain circumstances be reversed as regards their initial and end states. Some processes which are called reversible (e.g. reversible changes of state) cannot be reversed along the same path, but return to the initial state, if at all, by a different path. More-



over, one precise meaning of ' irreversible ', as applying to the increase of entropy in a closed system, is irrelevant to processes where the entropy is not defined owing, for example, to the absence of local equilibrium. Finally, the term ' irreversible ' has the disadvantage that it is associated with the attempts to explain irreversibility in terms of reversible laws, whereas here the aim is to advance towards a representation of apparent or approximate reversibility in terms of fundamental irreversible or *one-way* laws.

We therefore define *a one-way process*[1] *as any actual process in a finite system of nucleons*[2] *which never completes an exact cycle and moves towards a relatively stable equilibrium state, called the terminus.* This definition is not free from ambiguity but serves to indicate a realm of enquiry which, as we shall see, may be described as the theory of diminishing and vanishing parameters.

One advantage of this concept is that it does not imply the context of any particular theory, or the use of any special method of representation. It applies to actual processes, that is to observed changes in spatial arrangements, without requiring a decision whether the systems are closed or open,[3] and covers all processes—whether inorganic, organic or other—which display a monotonic movement towards equilibrium, whether or not the net result of the process may under other circumstances be reversed.

Planck[4] contrasted ' natural processes ', which actually occur and move towards equilibrium ; ' unnatural processes ', tending away from equilibrium which never occur in isolation ; and ' reversible processes ', which are limiting cases of passage in either direction between a continuous series of equilibrium states. One-way processes are Planck's ' natural processes ', in isolated material systems.

The class of one-way processes thus defined is wider than the class of entropy processes, but does not cover all processes displaying the asymmetrical relation of succession. The history of the cosmos, the nebulae, the stars, and the solar system, as well as organic evolution, all involve the relation of succession, but none of these processes is known to be leading towards a relatively stable equilibrium state. One-way processes, as here defined, are those possessing a stable terminus which is within sight, or can reasonably be predicted.

In statistical thermodynamics the increase of entropy is interpreted as a tendency towards states of higher probability. But this is a restricted interpretation, valid only in a theory which postulates certain *a priori* equal probabilities whose observational meaning is

[1] The concept of a one-way process was defined in *Nature*, 1949, **163**, 762, in a slightly more general manner, where references are given to the related ideas of Koehler, Sellerio, Renaud, and others.

[2] If one-way processes are basic then material (nucleonic) systems, rather than the stationary wave fields of lighter particles, must be treated as fundamental. The restriction of the basic concepts to systems of nucleons eliminates certain hypothetical unobservable processes, such as photon or electron wave packets diffusing to infinity.

[3] Provided that a state of relatively stable equilibrium is reached

[4] M. Planck, *Ann. d. Phys.*, 1887, **30**, 562

not clear. One-way theory, as a programme for research, is more general, assuming only the fact of a widespread tendency towards equilibrium.

Reversible processes have associated with them a quantity, energy, which satisfies a conservation principle. Similarly one-way processes are characterised by a signless variable which always decreases. In one-way theory conserved quantities are no longer treated as theoretically fundamental, but are replaced by, and represented as limiting cases of, a more general class of quantities, conveniently called ' diminants '. A *diminant* is defined as a signless variable, representing the deformation of a system from a state of stable equilibrium, which deformation continually decreases during a one-way process. Here the term ' deformation ' is used for a spatial (3D) asymmetry or non-equivalence which vanishes in the equilibrium state. A *fundamental diminant* is one representing the deformation of a system of nucleons from an equilibrium arrangement. Diminants are not localised parameters, like the temperature at a point, but collective or system parameters, like the total energy or entropy of a finite system. In certain limiting cases where the decrease of a particular diminant, D_c, relatively to the change of some other variable or diminant, is negligible, the diminant D_c can be treated as the equivalent of the constant energy-function of a conservative system.

Current theory employs a principle of microscopic reversibility,[1] while one-way theory proposes to use a basic principle of tendency. There is no necessary conflict between them regarding observations. They are contrasted methods for the analysis and classification of observations, and one-way theory is potentially the more general. This may easily be seen since reversible theory deals either with aged systems which have already settled down into cyclic, steady, or equilibrium states, or with transient stationary states which have not yet decayed, while one-way theory sets out to cover the processes of aging and decaying. Reversible theory is a first approximation to one-way theory, valid when certain dimensionless ratios are so small relatively to unity that the first variable term in a power series expansion provides an adequate approximation.

It is necessary here to correct a common misconception. In 1927

[1] But the principle of microscopic reversibility does not hold when account is taken of spin properties and magnetic fields ; cycles of processes then take the place of fundamental reversibility.

Eddington stated [1] that ' so far as physics is concerned time's arrow is a property of entropy alone '. This is misleading since entropy has only been defined for systems divisible into parts each of which is in equilibrium. The vast realm of actual irreversible processes in which atomic-molecular disequilibria are present lies outside its scope. What Eddington meant to say was : ' in the *known* physical laws time's arrow only appears as a property of entropy ', which is something very different. This is an example of a widespread unscientific carelessness in speaking of entropy. The following supposed applications of the concept possess no quantitative significance following unambiguously from the definition of entropy : the entropy of the universe at a specified moment, the entropy of a molecular region during an atomic or chemical change, and the entropy of an organic cell through which pulses of polarisation and depolarisation are passing.

If the conception of a diminant were entirely new it would be useless. Wherever the equilibrium state of a system is marked by an extremal value of some function of the parameters of the system, as is often the case, the signless difference of this function (Q_t) at time t from its extremal value (Q_m) is a diminant :

$$D_t = | Q_t - Q_m | .$$

The introduction of this term is desirable to emphasise a property common to all one-way processes, and to separate this property from the context of established theories, by implying that in the proposed theory diminants are more fundamental than conserved quantities. One-way theory exploits the assumption that every process is initiated by some quantitative difference which decreases in the course of the process, by treating these differences as theoretically fundamental. Moreover the term implies that every isolable process comes to an end. While cyclic processes may continue indefinitely (in idealised cases treated by theory) each one-way process is normally a *unit of process* leading from a threshold to a terminus.

The key problem in one-way theory is : *What are the conditions under which a stable, stationary, or nearly-stationary state appears in any system?* Or more generally: *When do nearly constant parameters appear?* Classical physics provided general criteria for equilibrium, but no explanation of the stability of systems of particular sizes or shapes. Quantum mechanics covers many stationary and stable

[1] *Nature of the Physical World*, 1927, p. 80. In *The Internal Constitution of the Stars*, 1926, p. 44, Eddington was more careful : ' The ultimate laws of nature (*so far as known*) leave the direction of time indeterminate . . . ' (My italics).

states, but does not provide elegant solutions. One-way theory should make explicit the conditions under which a stable state is produced as the result of a one-way process.

We shall now consider five important types of one-way process in physics and biophysics, starting with one-way processes and diminants which are well known (though not under these names) and passing to speculative examples where interesting problems arise.

(1) *Entropy Processes* (S), *displaying a tendency towards maximum uniformity, equipartition or entropy* (S_m) *under the diminant*

$$D_s = (S_m - S)/S_m$$[1]

representing a difference between scalars.

The concept of entropy is restricted to (i) systems divisible into states in equilibrium or where local discontinuities or disequilibria (fluctuations, molecular transformations, etc.) are negligible ; and (ii) non-equilibrium states which can be represented by a well-convergent series as small displacements from equilibrium states. This means that the entropy principle applies only where the process depends on a function of scalars, that is, it must not depend explicitly on the gradients or the time.

The next step should be to transform and generalise the entropy concept into a new function, explicitly dependent on local vectors, covering micro-deviations from equilibrium and changes in atomic-molecular-structural vectors (exact structure of discontinuous entropy production, Gibbs Free Energy and quantum entropy as functions of atomic structure, and the introduction of relaxation times).

Increase of entropy corresponds to increase of the number of possible molecular permutations consistent with the values of the fixed macro-parameters. Every added permutation implies that some diminant has vanished. The increase of entropy thus corresponds to the vanishing of diminants, the value of the entropy is a measure of the number of diminants that have already vanished, and the rate of internal entropy production (dissipation function) a measure of the rate of vanishing of diminants. The concept of entropy is restricted to those processes in which the diminants represent *differences of scalars* ; the required generalisation is to diminants representing *differences of vectors* (in atomic-molecular structure, etc.).

(2) *Processes leading to the formation of stable or relatively stable structures, displaying a tendency towards some extremal structure characterised*

[1] Diminants are best expressed in dimensionless form.

by minimum internal (potential) energy ($E_{min.}$) and by higher structural symmetry or completion, under the diminant $D_E = (E - E_{min.})/E_{min.}$ Examples are the formation of nuclei, molecules, crystals, states of uniform polarisation, magnetisation and aggregation.

No simple geometrical characterisation, for example in terms of the nucleons present, is yet possible of the property of ' completion ' which results in ' atoms with completed electronic shells ', or nuclei of certain types, being relatively stable. It is a weakness of the theories of this century that the extremal properties which determine the equilibrium configurations of particles have not yet been reduced to geometrical relations, but require the use of dynamical parameters (e.g. unobservable ' momenta ' in excited stationary states and ground states).

When a general geometrical criterion for structural equilibrium has been discovered, it will be possible to express the diminant D_E as a function of the relations of fundamental particles, such as nucleons. It may then be found that the fundamental expression for D_E involves a difference between vectors, in contrast to case (1) above.

(3) *Interactions.* (*Hypothetical one-way process.*) *The interactions between the traditional idealised reversible phenomena of fundamental physics may constitute a class of one-way processes, displaying a tendency towards equilibrium (in a system of nucleons) under a diminant* D_I *representing the deformation (excitation) of the system from its equilibrium arrangement.* On this view the ' interactions of lighter particle fields ' are to be represented as relaxation processes in nucleonic systems.

The main lack in current particle theory is a satisfactory treatment of complex interactions, all the established reversible processes being idealisations of actual irreversible phenomena, obtained by neglecting their interactions. While two-entity interactions at low velocities are reversible, multiple and high-energy interactions involve irreversible features. We may therefore ask : How is allowance to be made in the fundamental laws for these interactions? What correction terms have to be added to the reversible laws, or to their solutions, or what other transformation is necessary, so that the new expressions represent the underlying one-way process which automatically includes the interactions?

We here assume that this more comprehensive law, including the interactions, has the form of a one-way process leading towards equilibrium in a finite material system (system of nucleons). Where the process of interaction (A —B) involves a transfer of energy ($A \rightarrow B$),

the vector representing the energy transfer must be determined by some spatial asymmetry ($A \rightarrow B$) which vanishes when the transfer ceases in the terminal state. This asymmetry could be taken as the diminant of the interaction one-way process.

Consider the case of electron-photon interactions where a nucleon system provides the boundary conditions of the lighter particle fields. To reach the representation of the actual one-way process underlying the idealised 'electronic stationary states' of atomic systems the Schrödinger-Dirac differential equations (or their particular solutions) must be completed by the addition of a term representing the element of one-way process which is present in excited states, since these always possess a finite transition probability. The finite breadth of an excited state should never be neglected, for it is never theoretically trivial. On this view the observed phenomena are not stationary states, but *most-nearly*-stationary components of an underlying one-way process.

Thus the path towards a unification of these two approximations, the electron and photon fields, may perhaps be found by re-introducing the neglected one-way feature. The electron concept neglects the radiative interactions; the photon concept neglects similar interactions, particularly the changes in the electronic boundary conditions of the photon field; the correction of either should lead to an expression representing the one-way process (in a nucleonic system) to which these two stationary fields are approximations. Here a new theoretical problem has emerged: '*Can a one-way process be identified of which electron and photon fields are most-nearly-stationary components?*' which may prove more fertile than the search for an appropriate non-linear field equation.

This argument has a precise quantitative implication. In current theory the efficacy of the concept of stationary states is ascribable to the smallness of α ($\alpha \ll 1$), α determining the magnitude of the electron-photon interactions. And *the presence of one-way effects is regarded as due to the finitude of* $\alpha(\alpha \neq 0)$.

But in a future one-way theory this reasoning would be reversed and rendered more constructive, as follows: *The finite value of* α *is due to the fact that the stationary states are complementary components, only separable with a finite error, of a basic one-way process.*

The element common to both arguments is the assumption that the value of α expresses the relation of one-way to stationary-reversible phenomena, e.g. the ratio of decay rates to frequencies. This refers

to temporal relations only ; as regards spatial relations α determines the ratios of path curvatures to wave-lengths or sizes of atoms.[1]

Similar arguments apply to other interactions, but these cannot be considered here. In general we may say that the traditional methods based on reversible representations are only valid when the irreversible effect of their interactions is so small that it can be neglected, i.e. not introduced explicitly into the laws, but ascribed to special initial or boundary conditions.

(4) *Auto-catalytic formation of stable structures, in which a catalyst facilitates the replication of some structural feature or unit* (*inorganic auto-catalysis, action of genes and enzymes*). Special case of (2) above. Such processes display a tendency towards higher structural symmetry or equivalence in the system catalyst *plus* less stable structure on which it acts, under a diminant (D_σ) as yet unidentified.

Every process in which an existing structure (σ_1) impresses some feature of its own pattern on a less stable neighbour (σ_2), so that a new stable structure (σ_1') is formed identical to σ_1 (or to some part of σ_1) is necessarily a one-way process under a diminant of the form D_σ = some structural difference between σ_1 and σ_2. D_σ might be taken as a difference in the orientation of corresponding structural features, such as chemical bonds already established or in process of being formed. *Any* theory of identical replication of structural elements (inorganic or organic) must explicitly or implicitly employ a diminant representing a difference between vectors, which difference decreases and vanishes as the process of replication moves to completion.

(5) *So-called ' disorder—order ' transformations* (*in mixed crystals, etc., development of more highly ordered states in inorganic systems, protein systems, and the development and restoration of order or organisation in biological systems*). *Hypothetical class of one-way processes. These processes display a tendency towards higher structural order* (*in a sense not yet clarified*) *under diminants which may represent differences of structural vectors.*

Until recently it was widely supposed that there exist two widespread and directly opposed tendencies : towards ' disorder ' (entropy processes interpreted as tending towards higher dynamical disorder) and towards ' order ' (formation of regular molecules, crystals, etc. ; transitions towards ' more highly ordered ' phases ; and most organic processes). This view is now known [2] to be misleading. It

[1] *Nature*, 1950, **166**, 824

[2] Cf., for example, M. J. Klein, this *Journal*, 1953, **4**, 158, and J. M. Burgers,

rested on a confusion of multiple meanings of ' disorder ', ' order ', ' organisation ', etc. Part of this linguistic confusion can be dispersed by noting that both tendencies display a movement towards equilibrium, and are therefore potentially subsumable under one mathematical principle : the tendency for arbitrary parameters which represent freely variable differences or non-equivalences to decrease and vanish. In entropy processes these diminants are the thermodynamic forces ; in processes leading to the establishment of regular or ordered forms the diminants are structural non-equivalences not yet identified.

If we eliminate the ambiguity in the current use of the terms ' order ', ' disorder ', by defining a type of order, *parametric order*, whose measure is the smallness of the number of independent parameters required to represent the relevant features of the system, then both tendencies display a movement towards higher parametric order. Parametric order can only increase, or remain constant, in an isolated system ; it can never spontaneously decrease. Entropy is a statistical (scalar) measure of parametric order ; the as yet unknown laws of the simple processes of complex systems probably involve the concept of parametric order in a non-statistical (vector) representation. The concept already in use of the ' degree of order ' (proportion of atoms on their proper sites in super-lattices) can be regarded as a special case of parametric order.

One-way processes of type (4) and (5) concern the transformations of complex systems and are relevant to important bio-physical phenomena. Two of the most striking general properties of organisms are their *internal structural co-ordination as working units* and *the significant tendencies evident in their working*. If particle theory finds it necessary to employ the methods of a *collective finalism* (system diminants), another remnant of the theoretical boundary between the non-living and the living will have vanished.

L. J. Henderson in *The Order of Nature* gave a classic analysis of the relations of mechanism and teleology as these terms were understood in 1917. Today we can consider the more precise problem of the relation of quantum-mechanical reversible processes to one-way or finalistic processes. Vitalism assumed a significant one-way tendency in organic systems, but neglected structure ; quantum mechanics covers structure, but does not admit a one-way tendency into its basic postulates ; it is here suggested that a future theory must be

this *Journal*, 1954, **5**, 70. In the light of these clarifications the use of the term ' negative entropy ' in relation to organic systems requires reconsideration.

based on a way-way tendency revealed in structure. The open question is whether this unorthodox proposal can lead to a physical theory more powerful than those based on the isolation of reversible processes, which has been the preferred and successful method ever since the origins of mathematical physics.

We may now summarise the main theoretical problems arising from this survey :

Unification of Diminants. The principal aim of research in particle theory is the unification of the various particle fields. This presents many difficulties. An alternative is to seek a comprehensive theory of particle interactions and to ask : What is the most general type of diminant (D_G) of which all other diminants (structural non-equivalences, thermodynamic forces, etc.) and conserved quantities (forms of energy) can be represented as special cases? What geometrical properties must D_G possess if all known diminants and time invariants are to be derived from it?

Determination of nearly stationary parameters. Equilibrium states have hitherto been determined, in restricted fields, by minimum potential energy or by maximum entropy, and steady states of certain open systems by minimal rate of entropy production. But we can ask more generally : In any complex process under a diminant D_s, with component processes (A, B) under sub-diminants, D_A, D_B, what are the conditions under which the change ΔD_A in D_A becomes minimal relatively to the simultaneous change ΔD_B in D_B? These conditions are of general significance, for when $\Delta D_A/\Delta D_B \ll 1$, D_A may be treated as constant and A as separable from the rest of the system, within a small error.

Selection of dimensionless expressions for diminants. Where the laws are unknown complex observations may often be expressed in a condensed form by using dimensionless groups. What type of dimensionless variable is appropriate for representing the diminants of one-way processes? This problem links the present paper with last year's Address.

Any analysis of a branch of science undertaken for heuristic purposes must be guided by some principle which cannot be justified in advance. One should, however, demand of any such principle that on the one hand it lies close enough to established facts and methods, and on the other it is general enough to be potentially capable of guiding an enquiry that may occupy a long period. The conception of one-way processes under diminants of various kinds appears to satisfy these conditions.

SOME COMMENTS ON METHODS AND STATEMENTS IN PARAPSYCHOLOGY AND OTHER SCIENCES *

G. D. Wassermann

1 *Introduction*

During the last twenty years experimental parapsychology has made great strides. The development of experimental methods peculiar to that subject have raised numerous general methodological and philosophical questions, which have not all been adequately discussed. Sceptics found fault with the details of earlier experiments. Gradually experiments were so much improved and reprehensible errors of methodology so thoroughly stamped out that the sceptics found no more justifiable fault in the experimental set up.[1] Criticisms began therefore to shift to philosophical issues, which primarily concerned the *interpretation* of the results obtained. Three of the most important of these issues are: (1) whether parapsychological experiments are repeatable and in which sense they are repeatable, and also whether specific effects are predictable; (2) whether adequate control methods exist; (3) whether statistics is on logical grounds a reliable tool for assessing the significance of experimental data in this field.

Recently G. Spencer Brown has devoted his attention to these issues and in fact has challenged current interpretations of parapsychological experiments.[2] He has argued that in parapsychology, experiments are neither repeatable nor repeatable *at will*; that there exist no adequate control series; and finally that the data of parapsychology only throw doubts on the applicability of significance tests, and do

* Received 4.viii.54. This paper is based on numerous discussions of the writer with a colleague, Dr R. Harrop, to whom some of the leading ideas and some of the details are due. It was not found possible to present our partly divergent views satisfactorily in a joint paper, and it was therefore decided that one of us should write a paper presenting his own point of view. Many of the arguments I give do, however, owe much to the discussions I have had with Dr Harrop.

[1] S. G. Soal and F. Bateman, *Modern Experiments in Telepathy*, London, 1954

[2] G. Spencer Brown, 'Statistical Significance in Psychical Research', *Nature*, 1953, **172**, 154

not support customary interpretations in terms of telepathy, clairvoyance, and similar notions. It seems that some of his propositions, if accepted, could, by slight generalisation, invalidate many currently accepted demonstrations of phenomena in biology and psychology. For these very reasons much of the present discussion may be of concern to biologists, psychologists, and other scientists, and not only to parapsychologists. In fact, the extensive discussion,[1] which has recently arisen from Brown's arguments is believed to be of general interest and the present paper is intended as a contribution to it. Some of my conclusions may appear 'obvious'. Yet Brown's article in *Nature* [2] and his broadcast [3] suggest that the present comments are not superfluous.

I primarily wish to analyse the *logical basis* of Brown's statements. However, this discussion does not pretend to be exhaustive. Certainly, a general treatment of the topics touched on would require a book rather than a paper. Neither is there any intention of going into any details or discussions of experimental parapsychology that are not relevant to the logic of the issue.

My aim is to distinguish different *types of statements* made in psychology, biology, and parapsychology and to show that this distinction is essential when arguing about repeatability and predictability. Brown's views on repeatability will be repudiated because in the writer's opinion he has failed to make this distinction. After discussing the nature of control experiments very briefly, and unmasking an apparently erroneous assumption in Brown's arguments, I shall discuss the validity of statistics when applied to psychical research. Again, I have no ambition to discuss the foundations of significance tests. It is only my aim to suggest that the formalism of statistics can be reinterpreted. When this is done the usual interpretations based on the significance tests are seen to be not in the least invalidated by

[1] Soal and Bateman, op. cit.; Spencer Brown, op. cit.; Report to the First International Conference of Parapsychological Studies, Utrecht, 1953; Letter to *Nature*, 1953, **172**, 594; Letter to *J. Soc. Psych. Res.*, 1953, **37**, 210; 'Telepathy and the Laws of Chance', B.B.C. Broadcast, 17th February 1954; S. G. Soal, Report to the First International Congress of Parapsychological Studies, Utrecht, 1953; Letter to *J. Soc. Psych. Res.*, 1954, **37**, 246; S. G. Soal, J. M. F. Stratton and R. H. Thouless, Letter to *Nature*, 1953, **172**, 594.

[2] G. Spencer Brown, 'Statistical Significance in Psychical Research', *Nature*, 1953, **172**, 154

[3] G. Spencer Brown, 'Telepathy and the Laws of Chance', B.B.C. Broadcast, 17th February 1954

Brown's arguments. On the contrary, it will be pointed out that lack of adequate randomisation and similar effects suggested by Brown could not plausibly account for the known facts of parapsychology.

2 *Repeatability and Predictability*

It is not easy to give a general definition of the concept of repeatability. In experiments in which *measurements* are performed repeatability may be defined as follows. If similar sets of measurements, which were carried out under standardised conditions, are compared and it is found that the results of the measurements for each set fall within permitted (arbitrarily assigned) limits (e.g. statistical limits of confidence), then the results may be said to be repeatable. There are other situations also where repeatability of aphenomenon has apparently occurred but where no measurements were made. For example, Sir Wilder Penfield and others have electrically stimulated similar regions of the cerebral cortex of many of their patients, frequently obtaining similar responses. Class characteristics may in this case be used to delimit what we call a repeatable phenomenon. It is, of course, important to realise that irrespective of whether measurements can or cannot be performed, the permitted limits of the classes which determine the repeatable phenomena are always dependent on arbitrary conventions. Moreover, within these limits variations of the class members are permitted.

Evidently, according to this definition of repeatability, a phenomenon, even if it occurred only twice, was repeatable. Scientists, however, are not just interested in whether a phenomenon occurred more than once, but whether, when sufficient repeat-experiments were carried out the phenomenon occurred so *frequently* as to conform to prescribed standards of significance. In future when I refer to the repeatability of a phenomenon I shall invariably imply that the phenomenon was *significantly repeatable*, i.e. that the frequency of its occurrence meets prescribed standards of significance.

Science is ultimately devoted to the discovery of relations or classes of such relations among phenomena, or to classes of *similar* events or phenomena, and without repeatability such relations would not seem possible. Nevertheless, the need for repeatability does not imply that there is only one type of scientific statement that refers to repeatable phenomena.

It seems to me essential to distinguish at least *two types* of statements which refer to the repeatability of scientific phenomena. Often

scientists only state that there *was* repeatability of a phenomenon for long periods of time, but no prediction is made that there *will* be future repeatability of this phenomenon. Such statements may be called *historical-repetition statements*. For example, palaeontologists may state that repeatedly, generation after generation, there occurred dinosaurs which ultimately became extinct. This is an inference from repeated observations on fossil finds. However, few people would like to predict that living dinosaurs will be found next year in the streets of London. In fact, there is no logical need for any form of prediction in order to make the statement about the past creditable. The recognition that dinosaurs existed is based on *agreed* standards of significance, which in this case are not statistical but expressed by a majority opinion of palaeontologists. Similarly biologists may point out that certain bacterial cultures showed in the *past* no resistance to penicillin for long periods of time, during many generations, until suddenly mutant strains evolved, and thus an irreversible change occurred.

It is obvious that historical-repetition statements require no predictability of phenomena, since, by definition, they refer exclusively to the past and never to the future. These statements are therefore usually not of pragmatic value when applying science to forecasts of future situations ; but they may be of great indirect value when formulating theories (e.g. a theory of evolution). For example, it is certainly important for medical men to know that drug resistance may develop in mutant strains of bacteria, even if they cannot predict whether in an individual case such drug resistance will occur.

Perhaps the most important characteristic of historical-repetition statements is that they invariably refer to a class or classes which comprise only a finite number of elements. These statements therefore contain typical terms such as *some* or *sometimes*. To illustrate this we consider a few typical examples. Zoologists claim that *some* calves with five legs *have been* born. Yet no claim is made in this case that such a calf will ever be born again or that *all* calves will have five legs. Similarly, parapsychologists only claim that *some* subjects *sometimes have* shown for long periods of time an unusual guessing behaviour in card guessing experiments, which was significant by certain standards of significance. However, no claim is made that *every* subject when tested under similar conditions *will always* show such behaviour.

Psychologists, biologists, and parapsychologists deal with organisms which do not show a *static type of ordered behaviour*. A certain behaviour

pattern of an organism may be elicited with proper stimuli repeatedly during long periods of time (so as to be significant) and then, often quite unpredictably and sometimes suddenly, the usual stimulus may fail to elicit the familiar pattern. The decline of I.Q. with age above a certain age for the average of the population and the corresponding decline of brainweight are other examples of the change of human abilities and organismic properties with time. Musicians or mathematicians of genius, whose powers of expression suddenly or gradually declined are known, and the subjects in parapsychology experiments whose scoring ability peters out after thousands of successful runs are examples of the same type. We can always assert in such cases what *has* happened, or what faculties *did* exist, but we can rarely, with any reasonable confidence, predict what *will* happen. Historical repetition statements are therefore most appropriate to such situations.

The second type of statements which I shall consider are *generalisations*. Generalisations are statements which refer (usually) to a class or to classes which have an *infinite* number of *hypothetical* elements (e.g. infinite populations in statistics or possible values of continuous variables in physics). Generalisations are supported by a finite number of observed data and make *inductions* from these to an infinite set of hypothetical data. The ' exact ' laws of science (e.g. Ohm's law) are typical examples. Again, all probability statements are inductive. It is, of course, well known that generalisations have no predictive *certainty* and they only represent attempts to make reasonable posits about future events.

The contrast between historical-repetition statements and generalisations is striking. The former demand no predictability of phenomena and attempt no prediction, while the latter do. The former refer to a finite class whose elements represent *actual* observations, while the latter refer to an infinite class of hypothetical elements. Moreover, historical repetition statements refer only to the past, whereas inductions refer both to past and future. Perhaps most important of all, historical repetition statements may refer to a dynamic state of order which permits sudden, irreversible, unpredictable changes (e.g. new types of mutations), whereas generalisations require a well defined future behaviour of systems. Of course the preceding distinction does not exclude that an historical-repetition statement may contain a limited amount of generalisation. It may state, for example, that *all* members of the finite class which *were* made the subject of a past observation showed a particular characteristic class

feature. For example, *all* the dinosaurs *which were found* had a skeleton. However, the degree of generalisation of historical-repetition statements differs from generalisation statements in that it never includes such phrases as *must always* or *will always* or *tends always to show*.

Generalisation statements may only be used in biology, psychology, and parapsychology provided there are weighty reasons to assume that for a future time period, specified in the statement, certain predicted regularities will not change. Life insurance, for example, is based on inductive posits. Yet, even insurance companies may have to change their posits from time to time if, owing to *unforeseeable* causes, mortality rates should change. Thus there may be inductions with changing posits, which involve predictabilities limited to the period during which each posit applies.

In the light of the foregoing remarks we can now examine Brown's statements on repeatability and predictability in parapsychology. These are : (1) that, as a rule, identical experimental conditions do not bring about a repetition of results ; [1] (2) that the results are not repeatable *at will* ; [2] (3) that ' to demonstrate a causal tendency we must be able to say beforehand what sort of results we are going to get '.[3] He complains, for example, that in card guessing experiments ' . . . nobody can predict the coming of a successful guess and nobody knows when one has been made until the results are checked up afterwards '.[4]

It is seen that Brown's statements depend on his definition of the concept of ' demonstrability ' of phenomena. If we were to agree with Brown that only those phenomena are to be termed demonstrable which are repeatable *at will*, or whose causal tendency may be predicted beforehand, then certainly, parapsychological phenomena, and many other phenomena of science, would not be demonstrable. It must be realised that Brown seems to *define* only those phenomena as demonstrable, which permit *generalisation statements*. Thus by definition he automatically excludes all those phenomena from being demonstrable which require historical-repetition statements.

[1] G. Spencer Brown, ' Statistical Significance in Psychical Research ', *Nature*, 1953, **172**, 154

[2] G. Spencer Brown, ' Telepathy and the Laws of Chance ', B.B.C. Broadcast, 17th February 1954

[3] G. Spencer Brown, Letter to *J. Soc. Psych. Res.*, 1953, **37**, 210

[4] G. Spencer Brown, ' Statistical Significance in Psychical Research ', *Nature*, 1953, **172**, 154 ; ' Telepathy and the Laws of Chance ', B.B.C. Broadcast, 17th February 1954

Parapsychologists make essentially historical-repetition statements, which refer to the sort of causal tendencies that *were* observed. They do not therefore require statements as to what causal tendencies they are going to get in future experiments (generalisations). Secondly, parapsychological statements, being non-inductive, do not require indefinite repeatability or repeatability *at will*. However, parapsychologists can certainly point to extensive (significant) *past* repeatability in many experimental series, in the sense of historical-repetition statements. For example, Soal [1] found, with two remarkably gifted subjects, repeatability of guessing behaviour, typical for each subject, over long periods of time, in batches of 200 guesses in many thousands of trials. The guessing habits of his subjects exhibited a striking fine-structure of the spacing of the coincidence scores and this was found to be a repeatable phenomenon. Brown's statements that identical experimental conditions do not ' as a rule ' bring about a repetition of results is therefore repudiated by this important series of results and similar other extensive series conducted by other workers.[2] These series showed consistent behaviour during tens of thousands of guesses and even in some cases permitted successful limited inductive posits (comparable to those of life-insurance companies). In much of this work people set about purposely to demonstrate the presence of parapsychological ability and did succeed. Here the facts again are at variance with Brown's statements that such attempts to demonstrate ' telepathy ', etc., ' nearly always resulted in failure '. In one case, in fact, very striking repeatability occurred. Soal found that one of his subjects showed even after a lapse of several years repeatability of idiosyncratic guessing behaviour, when the same agent was used.[3] Of course, neither Soal nor anyone else ever claimed that there was *detailed* repeatability, guess by guess, nor is it claimed that every single run showed always the fine structure typical of the subject's scoring habit. Nevertheless, the typical fine structure recurred very strikingly and significantly (by usual standards) when similar experimental

[1] Soal and Bateman,op. cit.; S. G. Soal, 'The Experimental Situation in Psychical Research ', The Ninth Frederic W. H. Myers Memorial Lecture, London, 1947

[2] D. R. Martin and F. P. Stribic, ' Studies in Extra-Sensory Perception (I) : An Analysis of 25,000 trials ', *J. Parapsychology*, 1938, **2**, 23 ; ' Studies in Extra-Sensory Perception (II) : A Review of All University of Colorado Experiments ' ; same, 1940, **4**, 23 ; B. M. Humphrey, ' The Relation of Some Personality Ratings to E. S. P. Scores ', *J. Soc. Psych. Res.*, 1951, **36**, 453 ; G. Schmeidler, ' E.S.P. Performance and the Rorschach Test ', *J. Soc. Psych. Res.*, 1950, **35**, 323

[3] S. G. Soal, Letter to *J. Soc. Psych. Res.*, 1954, **37**, 246

conditions prevailed. If we only recognised experimental series, which showed guess by guess ' exact ' scoring level repeatability, when compared, then certainly we should have to exclude parapsychological phenomena from recognition. However, in that case we should also have to exclude all those parts of science which show experimental series capable of statistical fluctuation. This, I believe, would probably exclude the whole of science. Even physical repeat measurements hardly ever lead to exactly the same experimental figures.

Instead of comparing *individual* guesses, parapsychologists only examine critical ratios for a *whole batch* of guesses and try to see what sort of correspondences occur most frequently batch by batch. Their statements on repeatability therefore always refer to whole batches, and not to single guesses. Even for whole batches fluctuations are permitted and it is not claimed that repeatability means that every batch shows *identical* critical ratios. Instead the ratios are permitted to vary between permitted (arbitrarily assigned) limits.

It is crucial to realise that Soal only claimed that his two subjects showed their remarkable guessing behaviour in the *past* (historical-repetition statement). He did not claim that other experimenters under similar conditions *will always* find similar results, nor that any arbitrary subjects or agents would be suitable, nor that his subjects could continue their feats indefinitely. As his statements are not generalisations there is no logical necessity for such a claim to be upheld.

Brown's insistence that phenomena should be demonstrable *at will* is again another expression of his definition of ' demonstrability ' so as to exclude historical-repetition statements from being demonstrable. It is not difficult to give examples which illuminate the inadequacy of his criterion of ' demonstrability ' *at will*. Cosmic ray experiments provide an immediate instance. When physicists examine photographic plates which have been exposed to cosmic rays at high altitudes, they *sometimes*, though only rarely, find a particular sort of track which they call a meson track. If a sceptic were to ask a physicist to get him, in his presence, a meson track of a particular kind by exposing a batch of 100 plates for a given length of time at a high altitude, the physicist might be very embarrassed. The whole batch might not show a single track, so that the phenomenon is not demonstrable *at will*. But this hardly means that meson tracks have not been demonstrated. Earthquakes and meteorites in the atmosphere also cannot be demonstrated at will, but few people would deny that they

have been demonstrated. Again, a competent psychiatrist who may be an excellent hypnotist, may fail, when ordered to hypnotise a randomly chosen subject *at will*. Nobody would suggest nowadays that therefore hypnotism has not been demonstrated. People would only argue that hypnotism just does not work in such a fashion that results can necessarily be produced at will, and they would benignly smile at anyone who made such a demand. Yet only about 100 years ago medical opinion refused to recognise the existence of hypnotism on such grounds, and shortly before that people who accepted the existence of meteorites were made the laughing stock of society. It was then also held that such phenomena could not possibly exist, since they were not repeatable at will. It is seen that the fatuous demand (often made) that parapsychological subjects, if they have any gifts at all, should be able to predict the results of the stock exchange [1] is also based on the assumption that we can prescribe to natural faculties how and when they ought to work. A subject who is good at card guessing, even if he scores very highly, gives a poor performance compared to the feat required in predicting accurately a complicated series of events (e.g. the stock exchange).

Again, Brown's complaint ' that nobody can predict the coming of a successful guess and nobody knows when one has been made until the results have been checked up afterwards ' can now be compared with the statement that ' nobody who exposes plates to cosmic rays can predict the coming of a successful meson track exposure and nobody knows when one has occurred until the plates are checked up afterwards '. In other words, in order to demonstrate that a phenomenon *has* happened we do not have to have predicted beforehand *when* it should have happened, nor have we to have predicted the detailed structure of the phenomenon.

3 *Objectivity*

Scientists (particularly psychologists) have spent much effort to make experiments ' objective '. The doctrine of ' objectivity ' demands that, given a specified experimental set up, any experimenter should be able to obtain similar results under standard conditions and if desired in the presence of witnesses. This stipulation has held a dominant place in modern science.

[1] This demand was made by a speaker at the discussion on parapsychology at the British Association in Oxford in 1954 and was widely applauded.

Parapsychological experiments have revealed [1] a situation which from this point of view is of particular philosophical interest. It was found that the personality structure and attitude of *all* those who partake actively or *passively* in such experiments could be of great importance in deciding the outcome.[2] Thus the personality structure of experimenters, agents (i.e. the person who looked at the cards) subject and possible witnesses present could all be of decisive influence. For example, a change of agents in Soal's experiments could result in an immediate decline of successful scoring and, usually, resubstitution of the same agent would lead to resumption of the previous success. Even the detailed structure of the guesses depended on the personality of the agents used and changed for a given subject with the agents in a consistent manner.[3] Again, Soal found that only two out of 160 subjects tested by him could score persistently significantly above expectation, and these subjects scored in a manner depending on their personality. Other experiments revealed that a change of *experimenters* with the same subjects could change the outcome of the experiments, even if the experimenter played outwardly a passive part, by watching the experiments only. Some experimenters, with gifted subjects, produced consistently successful scores, while others were equally consistently unsuccessful with the same subjects (e.g. Soal and Rhine both experimenting with Mrs E. Garrett using the same technique failed and succeeded respectively). Finally the extensive work on the correlation of personality structure (determined by psychological tests) and scoring ability and scoring habits in parapsychological experiments have revealed notable results.[4] Gardner Murphy has stressed that work in this field has to be ' individualised ' because of these findings. The usual ' objective ' method is unsuitable, since the personality structure of the experimenter and others can influence the results. We have here what Murphy and others [5] have termed field phenomena, namely a complex interaction between a

[1] G. Murphy, ' Psychical Research and Personality ', *Proc. Soc. Psych. Res.*, 1949, **49**, 1

[2] B. M. Humphrey, ' The Relation of Some Personality Ratings to E.S.P. Scores ', *J. Soc. Psych. Res.*, 1951, **36**, 453 ; G. Schmeidler, ' E.S.P. Performance and the Rorschach Test ', *J. Soc. Psych. Res.*, 1950, **35**, 323 ; J. Fraser Nicol and B. M. Humphrey, ' The Exploration of E.S.P. and Human Personality ', *J. Amer. Soc. Psych. Res.*, 1953, **47**, 133

[3] Soal and Bateman, op. cit.

[4] Humphrey, op. cit. ; Schmeidler, op. cit. ; Nicol and Humphrey, op. cit.

[5] Murphy, op. cit.

multiplicity of personalities in a single experiment. It is probably only in the social sciences that we find situations of comparable complexity.

Any statement that repeatability has occurred in parapsychological experiments must therefore take this complex situation into account. It must be stated that conditions were similar, namely, that the same agents, experimenters, witnesses, and subjects were used, that the subject's state of health was similar, etc. Otherwise there is no justification for claiming repeatability, or even to object that there was no repeatability. It appears that Brown's statements, and similar accusations raised by others, that *as a rule* there is no repetition in such experiments, has overlooked the complexity of the situation. Brown has not qualified his statements by reference to particular experiments and without such qualifications his statements become meaningless, since many experiments were performed under different conditions.

4 *Controls*

It has been argued by Spencer Brown and others[1] that parapsychological experiments lack 'satisfactory control series'. He maintains that in psychical research, results deviating significantly from expectation occur in control experiments when experimenters are intent on preventing their occurrence and that they cannot therefore be excluded to order.[2] He particularly emphasises that 'control series in this work cannot be relied upon to give results which are any less curious and "significant" than the main series'. This suggests a demand that in control experiments the phenomenon to be investigated *must* never occur and hence that there must be no 'significant' results in the control series. Brown apparently believes that experiments which do not satisfy this requisite are of little value.

A simple example will show that this demand is neither logically required nor practicable. A recent investigation seems to suggest that cigarette smokers are more prone to develop lung cancer than non-smokers. Cigarette smokers thus formed one series (the main series) and non-smokers another series (the control series). However, even non-smokers will in many if not in all cases have been exposed to cigarette smoke at some time. Hence it cannot be claimed that their lungs could not possibly have been in (significant) contact with the substances associated with cigarette smoke. It can only be asserted

[1] Brown, op. cit. [2] Brown, Broadcast, as cited

that cigarette smokers and non-smokers were generally exposed to *different* concentrations of smoke. Thus, in order to assess the effect of some suspected causal factor, such as cigarette smoke, it is logically not required that the factor be present in one series (the main series) and *completely* absent in another series (the control series). In fact, in many if not in all experiments it is impossible to make sure that a given factor *X* whose suspected effect is investigated could have no significant effect in the control series or is completely absent from the control series. Scientific control series have apparently one main purpose, namely to establish a *comparison* between two (or more) series of experiments of which one may be the main series and the other the comparison series. Usually a comparison is made either of results obtained from similar systems under changed experimental conditions or of results obtained for different systems under similar conditions. All that matters, if a causal factor is to be demonstrated, is that (two or more) different comparison series show a repeated consistent (significant) *differential behaviour* under different specified experimental conditions. However, there is no need for one of the comparison series to show nothing 'significant' as far as the suspected factor is concerned. For example, if for a gas the volume is measured against pressure at constant temperature and a curve is plotted, and curves are obtained for different temperature values (one curve for each value of temperature) then the different curves may be taken as comparison series. Here the same causal factor (temperature) was present in each comparison series of experiments and affected the measurements, though each curve was affected *differently*. Thus all comparison series (control series, main series) may be permitted to show *significant* influence of the same supposed factor *X* (e.g. temperature) provided only that this factor consistently affects the different comparison series in a distinctly different manner.

Brown's complaint that significant results in control series have occurred in parapsychological experiments becomes now irrelevant. First of all, in many types of experiments when guesses of subject were matched against shuffled packs of cards for which they were not intended and the same guesses were also matched against the cards for which they were intended *persistent and consistent differential behaviour* in the coincidence scores was found.[1] Secondly, in other experiments (e.g. Soal's) where several significant comparison series emerged each comparison series was consistently distinguishable from any of

[1] Soal and Bateman, op. cit.

the others [1] (just as the gas volume curves above for different temperatures). In fact in all cases where this occurred feasible theoretical explanations have been suggested, although these are not important from the present point of view.

5 *Statistical Significance*

We come now to a point of major interest. The following discussion is partly of general interest and partly arises from an interesting logical situation which seems unique in parapsychology and which does not seem to arise elsewhere in science. Brown contends [2] that the evidence collected by parapsychologists looks suspiciously like observations which would render the formulae for statistical significance unreliable. His argument may be summarised as follows. He considers statistics as a *model*, which is derived from a set of postulates. One of these is the postulate of the random occurrence of certain events (which occur as specific values of a postulated ' random variable '). For example, parapsychologists usually assume that the cards looked at by the agent are ' randomised '. The postulate of ' randomness ' encounters the difficulty that it is not possible for a finite set of ' random numbers ' to give a test which could uniquely check them for ' randomness '. The usual empirical tests can only make sure that *specific types of orders* do not occur. Suppose now the empirical process of ' randomisation ' used by experimenters, were—in the absence of suitable tests—not as good as is sometimes believed. This, Brown argues, might have serious consequences. First of all, it could manifest itself in a pathological behaviour of tables of random numbers, such as those constructed by Kendall and Babington-Smith. The usual tests for ' randomness ' would not have discovered these abnormalities. As biologists, psychologists, and parapsychologists sometimes use such tables this might have serious repercussions. In fact, Brown has hinted that pairing of independent columns of such tables might lead to more significant coincidences than statistics would lead us to expect if the tables were really ' random ' and that the structure of such coincidences might in fact resemble the structure of the results obtained in parapsychology.

It is well known that many people felt very impressed by this possibility and thought that Brown had at last ' explained away '

[1] Soal and Bateman, op. cit. ; Martin and Stribic, op. cit.

[2] Brown, Broadcast as cited ; ' Statistical Significance in Psychical Research ', *Nature*, 1953, **172**, 154

parapsychology. However, to check Brown's suggestion Oram [1] has recently paired 100,000 random numbers taken from the tables mentioned. Many of Brown's claims were not confirmed and the results behaved just as the statistical model would lead us to expect, except for a significant decline effect.[2]

The attack on the random tables is only the first of Brown's logical arguments. He suggests further that in parapsychological experiments the means of randomisation might have been influenced by the very processes which parapsychologists claim to have discovered. For example, they claim to be able by mental processes alone to influence the fall of dice and to be able to control by thought the motion of micro-organisms.[3] This possible influence on the empirical means of randomisation, or any other cause which might lead to inadequate randomisation, might then conceivably invalidate the applicability of the postulate of randomness and thus liquidate statistics as a useful model in assessing the significance of parapsychological data. The way in which such a breakdown of statistics could manifest itself would be by the occurrence of certain significant deviations from expectation values, precisely of the type found in parapsychology. Thus, according to Brown, the data of parapsychology need not necessarily suggest telepathy or precognition, etc., but could simply mean that a breakdown of the applicability of the statistical *model* has occurred.

Logically this is a very interesting suggestion and seems, for the moment, to have captured the imagination of many statisticians, particularly those unfamiliar with the facts of parapsychology. Similarly, for some psychologists Brown's arguments opened a splendid opportunity to deny wholesale without further scruples facts which were inconvenient, since they apparently did not agree with accepted theoretical models. However, it was less readily realised that Brown's arguments could in principle raise serious doubts against the use of statistics in the interpretation of facts in any field of research (including psychology) which employs empirical means of randomisation.

[1] A. T. Oram, 'An Experiment with Random Numbers', *J. Soc. Psych. Res.*, 1954, **37**, 369

[2] Even if Oram's work had confirmed Brown's suggestions in every detail, this would not have proved in the least that parapsychological data may be accounted for by such spurious effects. This will become clearer in the subsequent arguments given in the text.

[3] N. Richmond, 'Two Series of PK Tests or Paramecia', *J. Soc. Psych. Res.*, 1952, **36**, 577

Brown's argument in fact depends on the crucial assumption that statistics, as used in practice *must be considered as a model*, and hence depends on its postulates. I wish, however, to challenge this assumption, which I believe to be superfluous, although it has tacitly been taken for granted by most statisticians. It is suggested that in parapsychology and in many other fields of research statistics need not necessarily be considered as a model.[1] Instead in certain applications statistics can simply be regarded as the source of convenient *rules of calculation* which enable certain data to be expressed in 'normal' form for the purpose of *comparison*. For example, in the application of statistics to parapsychology and other researches statisticians often calculate so-called critical ratios. To compute these they make use of standardised formulae[2] which may be considered purely as *algebraic expressions* and as such *in no way* depend on the postulate of randomisation. The validity of the use of these formulae does not depend on whether the means of randomisation (e.g. tables of random numbers, or chips drawn blindly from a bag) are 'perfect' or not. Nor does it matter for the purpose of computation by what logical arguments these formulae arose in statistics. Now, what really matters in parapsychology and other sciences is the *comparison* of critical ratios for different comparison series of equal size. In other words, the *relative* magnitude of critical ratios for comparison series but not the absolute value of the ratios for each series is the crux of the problem. It is, of course, true that many scientists have posited their claims on the absolute magnitude of critical ratios. Although this may be perfectly justifiable in individual cases, it is of little interest to the present discussion. I wish to stress that there is *no necessity* for relying on the use of absolute values which require the use of statistics as a model. It seems that most, if not all experiments can be interpreted satisfactorily by studying only the *differential behaviour* of critical ratios (or of other suitable 'significance numbers') for different comparison series. Brown's major contention that in parapsychology the axiomatics of statistics might be infringed becomes now irrelevant, since the *comparison* of critical ratios does not in the least require satisfaction of the axioms. Nor is it therefore of interest from the present point of view whether the subject can or cannot influence the means of randomisation. However, with the present method of interpreting statistical

[1] This point of view is primarily due to Dr Harrop.

[2] I consider only significance tests based on critical ratios, since for other tests similar arguments can be given.

calculations, it could still be possible that the method of computation causes the particular structure of the data, or that a preferential guessing pattern of the subject might favour the now permitted non-randomness of the target cards and thus produce the result. These possibilities will now be briefly examined. Both of them will be shown to be apparently incapable, either singly or in combination, to account for the data of parapsychology. It follows then that the new interpretation permits to re-establish the status of the customary parapsychological interpretations to that which they held prior to Brown's criticisms. In other words, *Brown's criticisms are insufficient to reject current interpretations.* This, of course, does not mean that I accept these interpretations necessarily as the best possible. It is also noteworthy that the present method of considering only the relative magnitudes of significance numbers avoids Brown's charge that parapsychologists are guilty of a policy ' whereby when there is reason to believe other causal agents have been eliminated, *all* statistically significant results are taken as evidence for E.S.P., and only non-significant results are cited as relevant for the applicability of the probability calculus '.

These accusations are only justified as long as statistics is considered as a model. The mere applications of rules of calculation requires no general justification of ' applicability ', and thus Brown's arguments no longer apply to this interpretation.

First of all let us suppose that lack of randomisation of target cards, combined with a preferential guessing pattern of the subject had caused the results of most experiments. Many experimenters found that some of the subjects tested by them persistently failed to score successfully, while only a few subjects under similar circumstances scored successfully for long periods of time in a *consistent and persistent* manner. It was found when the guesses of the successful and unsuccessful scorers were both compared with cards randomised by the same process as the target cards, but *for which the guesses were not intended,* that only the successful subjects showed a markedly higher score on the targets than on the not ' intended ' cards. It is difficult to see how lack of randomisation could have caused this differential behaviour ; the same lack of randomisation should be present in *both* comparison series, since the same methods of randomisation were used. It seems rather that the ' intention ' was instrumental in the occurrence of the differential behaviour. An alternative, of course, would be to assume that the subject affected the means of randomisation in such a way as to cause the coincidences. Since the subject was

invariably separated from the means of randomisation, this would require some process of the type suggested by parapsychologists, namely, a long distance correlation.

In Soal's experiments in particular the relative magnitude of the critical ratios of various comparison series used changed consistently with change of experimental conditions. Thus, a change of the speed of guessing or a change of agents was regularly and repeatedly accompanied by characteristic changes in the relative magnitude of critical ratios. Most significantly the level of success dropped completely to 'insignificance' when the agents only touched the backs of cards instead of looking at them.[1] If we assume that the guessers always *synchronised* their specific guessing pattern so as to suit the non-random order of the targets, this leads again to difficulties. First of all the guessing pattern would have to adjust itself *consistently* to the change of experimental conditions. But in this case it would again be queer that the scoring of guesses against card orders for which they were not intended and obtained by the same methods never yielded anything significant. This suggests that a preferential guessing pattern could hardly be the correct explanation. Moreover, the fact that the experiments only succeeded when the agents looked at the cards and the fact that the structure of the guess coincidence varied sensitively with the agent used, suggests strongly that there existed a *causal* relation between subject and agent. It is this causal relation which parapsychologists have called E.S.P. Of course, this leaves open the question whether this causal process is reducible to any of the types of biological or physical processes hitherto known to science.

It is also most unplausible to assume that the possible synchronisation between the subject's biassed guessing behaviour and the non-random order of the cards could produce such *consistent and persistent* results as occurred in the work of Soal [2] and the work of Martin and Stribic [3] and also of Pratt and Woodruff,[4] to quote but a few examples. The synchronisation would have to show always deviations of similar sign, since, for example, in the work of Soal and Goldney [5] the deviations for each batch were always positive for the most marked series of coincidences. This would mean that the bias of the non-random

[1] Soal and Bateman, op. cit. [2] Ibid.

[3] Martin and Stribic, op. cit.

[4] J. G. Pratt and J. L. Woodruff, 'Size of Stimulus Symbols in Extra-Sensory Perception', *J. Parapsychol.*, 1939, **3**, 121

[5] Soal and Bateman, op. cit.

order of the target cards would have to favour strongly one direction of deviation. This assumption would be incompatible with the fact that some of the comparison series achieved with the same methods of randomisation led equally consistently and persistently to smaller deviations. If, on the other hand, the synchronisations were only a temporary chance effect, which led sometimes to a positive deviation, then some later work should sometimes show negative deviations. However, the persistence of one-sided deviations in Soal's work, week after week, suggests that this was certainly not the case.

It is also most unplausible that the methods of computation should have given rise to any of these persistent effects. The method of computation (i.e. algebrical formulae) could hardly distinguish between different comparison series in a consistent and persistent manner.

Finally, the correlations between personality structure and scoring habits and between the subject's attitude and his scoring habits, which were discovered persistently and consistently in the experiments of Humphrey,[1] Schmeidler,[2] and others could in no way be explained as being due to a lack of randomisation. For example, when the subjects were divided according to two attitudes *A* and *B* into two groups, *A* as a group would score positively above expectation and group *B* negatively below, showing thus a persistent and consistent differential behaviour of sign, provided the same experimenter was used. In these experiments all subjects of both groups directed their guesses simultaneously towards the *same* targets. Moreover, the subjects did not know what method of randomisation was used, so that there could be no learning process involved. Other experiments showed that psychological factors, such as a change of stimulus size or of 'interest' could produce a repeated and immediate correlated response in the subject's scoring.[3] These evidently *psychological* relations suggest strongly that we are here confronted with genuine psychological processes, which though at present no better understood than hypnotism, represent some thing very different from a statistical freak. In these experiments the faulty means of randomisation would have to be so biassed as to favour always the appropriate personality types, which seems most unplausible.

There are also many people, who argue, probably with justification (e.g. Soal and Thouless) that there are good reasons why statistics is

[1] Humphrey, op. cit.
[2] Schmeidler, op. cit. ; Soal and Bateman, op. cit.
[3] Humphrey, op. cit. ; Murphy, op. cit.

even applicable as a model in parapsychology. In particular they point to the existence of very large critical ratios, which they believe could not be due to a lack of randomisation on account of the null results in cross checks. While I do not deny the importance of their conclusions, I stress that the present considerations are independent of theirs.[1]

[*Note added in proof.* Since this paper was written an important paper was published by G. L. Mangan (*J. Parapsychol.*, 1954, **18**, 209). He obtained a highly significant differential behaviour in psycho-kinesis experiments with dice ; the results changing consistently with a change of intention of the experimenter. Like the Soal (etc.) experiments this work does not seem to be explicable in terms of Brown's hypotheses.]

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[1] The writer is indebted to Dr R. H. Thouless, Dr S. G. Soal, Professor J. M. Stratton, F.R.S., and the editor and referees of this *Journal* for helpful criticisms.

STRIFE ABOUT COMPLEMENTARITY (II) *

MARIO BUNGE

5 *Are Atomic Phenomena Unanalysable ?*

WE saw (secs. 2 and 3) that, for Bohr and Rosenfeld, the observed system and the instrument of observation form a whole, a ' sealed unit ' defining the phenomenon. ' Any atomic scale phenomenon must thus be conceived as a whole ; any attempt to apply to it the same kind of analysis as in classical physics would simply make it vanish. The word ' atomic ' here resumes its etymological sense with a more subtle connotation.' [1] That is to say, the atomic scale phenomenon is regarded as a whole in the sense of holistic philosophies—a totality that cannot be rationalised and hence represents a limit, a *non plus ultra*, to human knowledge. For instance, every energy change in an atom has to be regarded as elementary or atomic, because the forever indivisible quantum is involved in it ; [2] and, as Bridgman once said, ' it is meaningless to penetrate much deeper than the electron ', for there is really nothing within it.

This modern version of atomism is as mechanistic as ancient atomism and as irrationalistic as any obscurantist world outlook. The irrationalist feature lies in the claim that wholes are unanalysable, that their analysis and understanding is forever beyond all human possibility. This irrationalist aspect of the official philosophy of quantum mechanics was recognised by Bohr himself, when he wrote that the ' quantum postulate ', according to which every atomic process exhibits a character of ' individuality ' or wholeness, is an ' irrational element '.[3] Elsewhere, after describing an electron diffraction phenomenon, Bohr stresses the universality of such an irrational totality (which he calls individuality) :

> The impossibility of a closer analysis of the reactions between the particle and the measuring instrument is indeed no peculiarity of the experimental procedure described, but is rather an essential property

* Part I of this paper appeared in the May Number.

[1] Rosenfeld, *SC*, p. 395. See also ' L'évolution de l'idée de causalité ' (referred to below as *EIC*) *Mém. Soc. Roy. des Sciences de Liège*, 4e. ser., 1942, **6**, 59–87, p. 59 [2] Bohr, *LL*, p. 246 [3] Bohr, *TA*, p. 6

> of any arrangement suited to the study of the phenomena of the type concerned, where we have to do with a feature of *individuality* completely foreign to classical physics.[1]

This *a priori* impossibility of a finer analysis, of the discovery, so to speak, of the fine structure of the disturbance *caused* by the means of observation, was stressed even more sharply in Bohr's last philosophical paper, where we read that, ' in quantum mechanics, we are not dealing with an arbitrary renunciation of a more detailed analysis of atomic phenomena, but with the recognition that such an analysis is *in principle* excluded '.[2] In other words, it is not an arbitrary ' metaphysical ' ukase that forbids us to open the sealed unit constituted by the atomic ' phenomenon ' : now science itself dictates the ukase—*scientia dixit.*

Thus, for the upholders of the usual interpretation of quantum theory, atomic phenomena and their observation are no further penetrable. Instead of atoms of matter, or atoms of motion, we now have atoms of knowledge. This does not mean that the quantum philosophers share the Kantian prejudice of the unknowability of the *Ding an sich ;* no, they simply say that nothing is to be known beyond the phenomenon, that the phenomenon is the last stop *in res* as well as *in mente.* This irrationalist attitude was excellently expounded by Schlick, the late leader of the Vienna Circle, in his last paper, where we read : ' Quantum physics teaches inexorably that the detailed prediction of future events is in principle impossible. Hence, it sets upon the knowability of nature an unsurpassable limit. This is just the limit of the possibility of causal prediction.' [3] But this unknowability does not merely consist of the impossibility of knowing something existing *in res*, objectively though in a temporary or even forever hidden way ; such processes, elucidates Schlick, are not hidden, they simply *are* not, there is nothing beyond the unsurpassable limits set up by quantum mechanics.

According to positivism, quantum mechanics imposes a *limitation* upon our knowledge and at the same time it gives us a *complete* description of all there is to know. On a materialist theory of knowledge this would be contradictory ; on the positivist epistemology it is not. Schlick explains this point, repeating Bohr's contention [4] that quantum

[1] Bohr, ' Can quantum-mechanical description of physical reality be considered complete ?' (referred to below as *QMDPR*), *Physical Review*, 1935, 48, 696-702, p. 697

[2] Bohr, *DE*, p. 235, Bohr's emphasis

[3] Moritz Schlick, ' Quantentheorie und Erkennbarkeit der Natur ', *Erkenntnis*, 1936, 6, 317-326, p. 317

[4] Bohr, *QMDPR*

mechanics is a complete description of physical reality :

> The quantal laws do honour to the pretension of a complete, exhaustive description of nature in the sense that in principle they say *everything* that there is to say in any language about any natural process. And thus in general : when we say that according to the principles of quantum physics the knowability of nature is anyhow *limited*, this must never be understood as meaning that there is still something beyond the limit, something that will forever remain *hidden* from us. We have not to do here with a limit between known and forever unknown natural laws ; the limit of knowability is at the same time the limit of the lawfulness (*Gesetzmässigkeit*) of nature.[1]

In sum, positivists hold the ancient tenet that nature and knowledge are so to say finite, for they would be composed of ultimate, unanalysable atoms—this time atoms of a funny sort. Scientific materialists, on the other hand, reject ultimate atomism, which is a feature of mechanism, and the irrationalist dogma of the unanalysability of wholes, asserting instead that nature and the knowledge of nature are qualitatively infinite and inexhaustible. They maintain that at every level there are wholes which are so tightly knit, so predominantly determined by their inner motions and the interconnections of their parts, that they actually behave as atomic at their level ; but that there is nothing that guarantees us that these atoms are undecomposable at other levels, so that they may sometimes be regarded as undivided, but not as indivisible. This holds in particular for the unit formed by subject and object. In sum, scientific materialists reject atomism as an *ultima ratio* and the correlative assertion of the exhaustibility of knowledge.

It is clear why scientific materialists do not accept ultimate limits, as clear as why positivists are pleased in inventing them : it has always been subversive to push research forward, to demand an explanation for everything, to assert that no explanation is final. On the other hand, the positivist acceptance of facts as they are, the irrationalist dogma that there must be some unsurpassable *a priori* limits on knowledge, has always been the conformist attitude. This attitude of resignation, this basically unscientific and conservative attempt to fix *a priori* the scope of research and the depth of explanation is a favourite idea of the quantum philosophers and has been explicitly stated by Bohr in the following words : " every analysis of the very concept of " explanation " must always begin and end with a resignation regarding

[1] Schlick, op. cit., p. 319

the understanding of our conscious thinking activity.' [1] This attitude reminds one of Leibniz's remark on Descartes's belief that curves could not be rectified : he made this mistake, said Leibniz, out of sheer conceitedness, measuring the forces of the whole posterity by his own forces.

6 *Hypotheses non Fingo*

' No hypothetical element enters into Bohr's interpretation ' [2] of quantum mechanics, writes Rosenfeld. It is not clear whether he means here the popular or some of the technical connotations of the word hypothesis ; in either case his contention is wrong. First of all, the so-called principle of complementarity is itself hypothetical in every sense, because it is a more than hazardous conjecture (which is the popular connotation of ' hypothesis '), because it is not directly verifiable (being only one possible interpretation of Heisenberg's relations), and because it is used as the starting point for a host of inferences ; to such an extent that, we are told, ' If we consider it from the standpoint of epistemology, we may be led to modify the theory of knowledge '.[3] The only meaning that might validly be attached to the enigmatic sentence reproduced at the beginning is that complementarity is not a physical but a philosophical hypothesis, as is proved practically by the fact that not a single formula nor a single experiment have been derived from it.

To demonstrate that complementarity is an hypothesis, or a set of hypotheses, and not a factual statement, it will suffice to recall that it is based on several other hypotheses, so that its hypothetical character is of second degree. In fact, the so-called principle of complementarity and the whole philosophy connected with it rest on at least the following hypotheses :

(i) ' The present form of quantum theory furnishes the most complete possible description of physical reality ; in particular, the wave-function furnishes a complete specification of the state of an individual micro-object.' This hypothesis is false, I say, because no theory of reality has ever been verified to be complete nor can be complete in the same sense as a mathematical theory can be complete ; there can be complete theories of ideal objects because these have *by definition* a finite number of qualities. No physical theory could be complete on account of the following axioms underlying every scientific endeavour and confirmed by its failures and successes :

[1] Bohr, *LL*, p. 250 [2] Rosenfeld, *SC*, p. 396 [3] Rosenfeld, *SC*, p. 394

(*a*) nature and every one of its parts is inexhaustible, actually as well as potentially (as regards the parts, in themselves as well as concerning their infinite interconnections) ; (*b*) nature is an interconnected whole, so that the complete specification of a single object would require the complete specification of the whole universe, which is at least practically impossible ; (*c*) knowledge, as its history shows, is as inexhaustible as its objects ; (*d*) we are limited to a finite number of variables (in general, to a finite set of symbols with a finite number of properties), whereas the complete specification of every bit of matter would presumably require an infinite number of variables.

(ii) ' Heisenberg's uncertainty relations (which are an axiom of matrix mechanics and a theorem of wave mechanics, and which are just what the " complementary mode of description " aims at interpreting) are coeternal with quantum mechanics ; no modification of the letter is conceivable which would permit the simultaneous definition of the position and the momentum of a micro-object.' This assumption is clearly as frail as the first, belonging as it does in the same class of metaphysical assertions.

(iii) ' Physics does not deal with " autonomous attributes of the object " but with possible observations and with observables.' This, which is called the principle of observables, is a form of the Berkeleian ' to be is to perceive or to be perceived ', which I think I have refuted elsewhere.[1] Besides being philosophically wrong, the principle of observables contains the germ of its own destruction because, as it is well known,[2] it requires—if one wants the theory to be thoroughly ' operational '—the simultaneous assertion of the converse proposition, namely, ' An arbitrary quantum-mechanical observable, i.e. an arbitrary linear Hermitean operator, is physically observable '. The least that can be objected to this is that it is an unverified statement—which, hence, ought not to integrate a positivistic theory ; for it is plainly impossible to verify that experimental set-ups exist which would permit the measurement of the attributes represented by the operators which our fancy could define.

(iv) ' Atomic energy changes are elementary, or atomic in the etymological sense of the word " atomic ". The quantum is forever indivisible ; in particular, we shall never be able to analyse the exchanges of momentum and energy, so that the limit on knowledge

[1] Mario Bunge, ' New Dialogues between Hylas and Philonous ', *Philosophy and Phenomenological Research*, 1954, **15**, 192

[2] P. A. M. Dirac, *The Principles of Quantum Mechanics*, Oxford, 1947, p. 37

set up by Heisenberg's relations are unsurpassable.' This hypothesis is mechanistic, because it involves an ultimate atomicity, and it is irrationalist, because it asserts an *ignorabimus*. It is certainly true that the quantum of action symbolises a certain level of reality, the atomic level, which is a hard shell to pierce ; but every fact in the history of knowledge, as well as numerous hints coming from the lower levels not covered by the present form of quantum mechanics, suggest that the assumption of such an ultimate irreducible atomicity is wrong ; in any case, such an hypothesis is untestable.

(v) 'The finite or "atomic" interaction between subject and object, the unavoidable and irreducible link of every atomic object with its observer, forces us to abandon the ideal of objectivity and, as a consequence, every form of causality.' This assumption is wrong, in the first place, because there is a consistent *causal* interpretation of the present form of quantum mechanics [1] which is at the same time realistic, and which *retains* the indivisibility of the quantum. In the second place, because the subject-object interaction is, as any other interaction, a *proof* of *some* sort of causality ; only if measurements did not in the least disturb the objects of observation would the causal law cease to be valid. In the third place, because no single theory, however successful, could force us to forego causality, which is the key to scientific explanation ; at most we might be forced (actually we are forced) to abandon temporarily the hope of *predicting* with arbitrary accuracy the outcome of phenomena within a certain level ; but even such a temporary acknowledgment of a limitation upon *epistemological* determinism does not in the least injure *ontological* determinism, i.e. the principle that all things are interconnected with each other in a precise way, regardless of our ability to disclose the form of these connections.

(vi) 'There are two and only two ultimate categories by means of which we can describe atomic experiments : waves and particles, which must always be conceived of in the classical way, though as concepts having a limited range of validity.' This assumption, that particle and wave are the only possible forms of matter, is arbitrary ; nothing warrants that in the future other forms of matter shall not be discovered ; moreover, it is a noxious assumption, for it blocks the way to further advances. The most that might be said is that, up to now, we have not been able to transcend this conceptual limitation.

[1] Bohm, *IQT*

Thus, it is not true that ' the complementarity idea is first of all the most direct expression of a fact ',[1] it is not true that Bohr does not feign hypotheses when he speaks of complementarity. What is true is that the doctrine of complementarity is not a physical one—nor a right one.

7 *Complementarity in Classical Physics ?*

Following Bohr's suggestion,[2] Rosenfeld wants to extend complementarity to classical physics—which shows once again that complementarity is not a necessary outcome of modern atomic physics but a philosophical tenet. The idea of this extension is that in both fields our knowledge is imperfect, the nature of measurements preventing us from uniting all data in a unique conceptual model. Thus, we are told [3] that the microscopic and the macroscopic or thermodynamical aspects of the evolution of a physical system stand to each other in a relationship of complementarity ; in other words, the variables energy and temperature would be complementary to each other. The grounds for this conclusion are the following : [4] (1) energy is a mechanical variable (what would Ostwald have said of this ?), because it serves to define a system in statistical mechanics ; whereas temperature is, like entropy, a thermodynamical variable by means of which we perform the thermal description of the same system that is described mechanically by means of energy ; (2) energy and temperature are complementary to each other because, if one measures energy, thereby fixing its value, one is not able to ascertain the value of the temperature with arbitrary accuracy ; and conversely, the equalisation of temperature that takes place when we intend to measure it produces several possible energy distributions, that is, a latitude in the value of the energy—and this is interpreted in the sense that the system has no definite energy, or is in no definite energy state.

It is clear that this extension of complementarity is a consistent application of the basic epistemological principle of subjective idealism : *esse est percipi*. The system *has* no temperature, it is argued, if the observable temperature is not being measured ; it *has* energy only when we attempt to measure it (Destouches would say when we have

[1] Rosenfeld, *SC*, p. 396

[2] N. Bohr, ' Chemistry and the Quantum theory of atomic constitution ' (referred to below as *CQT*), *Jour. Chem. Soc.*, 1932, pp. 349-384

[3] Rosenfeld, *SC*, p. 398

[4] Rosenfeld, *SC*, and ' The foundations of statistical mechanics ', lecture, São Paulo (Brazil), August 1953

the intention of measuring it). It is also clear that one of the premisses of the argument, namely, that energy is an exclusively mechanical variable and temperature an exclusively thermal variable, is wrong; in fact, both can be introduced in a phenomenological (non-mechanical) way for matter in bulk, as is done in thermodynamics; or in an analytic way, as is done in statistical mechanics. Furthermore, the thermodynamic and the microscopic descriptions do not provide 'aspects' on an equal footing, as Rosenfeld assumes, but different levels of analysis disclosing correspondingly different levels of matter, each characterised by specific qualities (for example, temperature has a sense only for large aggregates of atoms). And, since no relation of complementarity is conceivable between concepts and conceptual systems referring to different levels, the microscopic and the thermodynamic descriptions cannot be regarded as complementary to each other. (It might be argued, in favour of Rosenfeld, that mechanism ignores levels.)

But all this is a trifle compared with the following discovery of the idealistic interpretation of statistical mechanics: the irreversibility of natural processes taking place in matter in bulk would be a consequence of our ignorance. Here is the proof demanded by the sceptical reader: 'The irreversibility arises from a statistical element superimposed upon the elementary laws, namely, the incompleteness of our knowledge of the initial conditions which determine the evolution of the system in virtue of those laws.'[1] This is an old idea of Bohr,[2] who more than twenty years ago stated that irreversibility is not a quality of processes at the macroscopic level (not a quality of wholes that is absent in each of its parts taken isolated from the others)—but a property of *our* description of them. Irreversibility, he explained, 'does not mean that a reversal of the course of events is impossible, but that the prediction of such a reversal cannot be part of any description involving a knowledge of the temperature of the various bodies'.[3] The same would happen in quantum mechanics: there is reversibility in the laws of motion, but

> essential irreversibility in the physical interpretation of this symbolism. In thermodynamics as well as in quantum mechanics the description contains an essential limitation imposed upon our control of the events which is connected with the impossibility of speaking of well-defined phenomena in the ordinary mechanical sense.[4]

[1] Rosenfeld, *SC*, p. 397
[2] Bohr, *CQT*
[3] Bohr, *CQT*, p. 376
[4] Bohr, *CQT*, pp. 376-377

Now, this supposed essential, inherent, unsurpassable limitation upon knowledge (exemplified by the ignorance of the initial values) seems to have quite magic effects : first, it produces irreversibility ; second, it produces—a deeper knowledge. As to the first effect, it is plain : it is just our imperfect knowledge of the initial conditions ' which defines the thermodynamic behaviour of the system '.[1] Thus, according to Bohr and Rosenfeld, irreversibility would not be an objective feature of material systems at the macroscopic level, a result of a given type of interactions between its parts, but a result of the limitation of our analysis of them. As soon as we improve observations, Rosenfeld maintains,[2] irreversibility disappears ; so that, if we want to take it into account, we have to stop our analysis superimposing on dynamics an irreducible ' statistical element ' (namely, finite cells in phase space). This resignation to ignorance, as Born [3] puts it in a similar presentation of this problem, far from being temporary is ' fundamental and inevitable '. And this is the second magic effect of ignorance : that it gives us a wonderful body of laws of nature in the form of statistical mechanics ; if further amplified by adding the postulate of the impossibility in principle of knowing the details of the microscopic behaviour of the system, then this peculiar brand of ignorance gives us an even deeper level of knowledge, namely, quantum statistics. How magic this sort of ' dialectics ' is !

To sum up, Bohr and Rosenfeld's idealist interpretation of thermodynamics and statistical mechanics contains the following philosophical hypotheses : (1) the idealist (or operationalist) axiom that a system has no energy (or no temperature) as long as we do not measure it ; (2) the idealist inference that irreversibility, that conspicuous trait of nature, is a consequence of our ignorance of details—which might be called the paradox of haughty humility ; (3) the curious opinion that, the more we ignore, the more we know—which is the paradox of the *docta ignorantia ;* and (4) the mechanistic principle that chance is irreducible and, moreover, the basic mode of behaviour, for phenomena have, according to Rosenfeld, ' a proper statistical character, inherent in their nature and accordingly irreducible '.[4]

[1] Rosenfeld, *SC*, p. 397

[2] Rosenfeld, lecture mentioned above

[3] Max Born, ' Le second principe de la thermodynamique déduit de la théorie des quanta ', *Annales de l'Institut Henri Poincaré*, 1949, **11**, 1-13, p. 6

[4] Rosenfeld, *SC*, p. 397

8 *Is Complementarity the only possible Rational Interpretation ?*

Rosenfeld claims that ' complementarity appears as the only possible rational interpretation ' of quantum mechanics.[1] Moreover, that ' The first point to realise is that the conception of complementarity forces itself upon us with logical necessity '[2]—as if it were a theorem, not a hypothesis. The aprioristic, dogmatic character of this contention is still more clearly shown in the prophecy that ' Whatever form the future theory will take, it will have to embody complementarity as a limiting case, just as complementarity itself embodies classical determinism '.[3] At the end of his allegation, Rosenfeld grants that ' complementarity is just a stage which we shall soon have to leave behind us '[4] in a sort of ' dialectical ' negation because, we are told, ' the future theory will reinforce complementarity by fixing its place within a still wider synthesis '.[5] Thus, first we are told that complementarity is the dialectical negation of determinism, which henceforth enjoys only a limited validity, namely at the macroscopic level ; now we are told that ' the ' future theory, far from dialectically negating complementarity, will *reinforce* it—which sounds like a mechanistic interpretation of dialectics, in so far as it suggests that the negated or sublated term remains unaltered, only its context changing with its negation. If applied to sociology, this view of dialectical negation might produce funny theories.

This strange prophecy on the *future* of quantum mechanics, presumably originating in supernatural revelations, is offered just as an antidote against the deterministic interpretations of quantum mechanics which have been recently advanced, and which, according to Rosenfeld, ' are all doomed to one fate '.[6] Why ? Because they are less rational than ' the only possible rational interpretation ' ?[7] Presumably. But, if this is so, we are faced with a new paradox, which may be schematised as follows : (1) the orthodox interpretation is the only rational one (and the only *possible* rational one at that !)—but, as Bohr[8] and Rosenfeld[9] have repeatedly asserted, it sets a limit upon rationality, upon the understanding of facts, it leaves an irreducible irrationality. (2) The non-orthodox interpretations are tacitly accused of being non-rational or at least less rational than the orthodox one[10]—whereas

[1] Rosenfeld, *SC*, p. 399
[2] Rosenfeld, *SC*, p. 394
[3] Rosenfeld, *SC*, p. 402
[4] Rosenfeld, *SC*, p. 408
[5] Rosenfeld, *SC*, p. 409
[6] Rosenfeld, *SC*, p. 403
[7] Rosenfeld, *SC*, p. 399
[8] Bohr, *TA*
[9] Rosenfeld, *EIC*
[10] Rosenfeld, *SC*, p. 400

they endeavour to transcend the limits set upon the rationality of atomic phenomena by the official philosophy of quantum mechanics and, at least as far as Bohm's causal interpretation [1] is concerned, Rosenfeld himself acknowledges that it is self-consistent.[2] As Kierkegaard said, religion is made of absurdities and paradoxes—and that is why it must be believed.

Let us take a look at one of those ' doomed ' deterministic interpretations. We shall limit ourselves to Bohm's work, which according to Rosenfeld ' is very cleverly contrived ' [3] and which is just the one that provoked the present rain of attacks on that author as well as on de Broglie and other physicists that have dared to object to the orthodox faith. In my opinion, Rosenfeld makes two basic mistakes in his short exposition of Bohm's ideas. The first is his contention that Bohm has given a description of atomic phenomena ' entirely in the spirit of corpuscular mechanics ', regarding the de Broglie wave as merely ' an auxiliary concept '.[4] It is enough to glance at Bohm's papers to see that he explicitly *postulates* the objective reality of the ψ-field, as well as the objective reality of the particle aspect of micro-objects. Now, it is astonishing that, of all the persons in the world, it is Rosenfeld who should criticise the hypothesis of the unreality of the de Broglie waves. It is just in the Bohr-Heisenberg interpretation that they are regarded as merely a conceptual tool (not even as a conceptual model of reality) enabling one to calculate probabilities —whence their name of ' probability waves ' or even of ' waves of knowledge ', as well as the absurd expression ' interference of probabilities '. In the orthodox interpretation we have neither particles nor waves, but only a pair of ' conjugate concepts '.[5] It is Bohr, not Bohm, who regards the wave-functions as merely ' symbols helpful in the formulation of the probability laws governing the occurrence of the elementary processes '.[6] In general, the formulae of quantum theory are regarded in the orthodox interpretation as statements about observable ' phenomena ' which do not assert anything about the real world. Why then should Rosenfeld dislike Bohm's non-existent elimination of the ψ-waves from reality ?

The second important mistake made by Rosenfeld in his hasty critique of Bohm's interpretation is his contention that, after all, it ends in complementarity, because when one measures the ' hidden parameters ' describing the real position and the real momentum of the

[1] Bohm, *IQT*
[2] Rosenfeld, *SC*, p. 403
[3] Rosenfeld, *SC*, p. 403
[4] Rosenfeld, *SC*, p. 403
[5] Rosenfeld, *SC*, p. 395
[6] Bohr, *CQT*, p. 370

electron, one falls back into the uncertainty relations. This is wrong, because in the causal interpretation those parameters describe objectively existing qualities, that is, qualities possessed by the electron even when we shut the lab and go to sleep—whereas in the usual interpretation they exist only in so far as they are ' conjured up ' by the experimenter (to use Rosenfeld's words [1])—who is supposed to act as a magician. Of course the experimental determination of these attributes is subject to error, since we know nature best by modifying it—but this time the uncertainties are regarded (1) as a practical limitation, not as an eternal curse, (2) as effects of a *real disturbance* exercised on *real* objects. In the usual interpretation we cannot say that the instruments disturb the object, for this implies that the latter exists objectively ; that is to say, the measurement is not regarded as producing a real disturbance in the original path and velocity of the micro-object, it is deemed to produce only a discontinuous and unpredictable change in the wave function (from a general state into an eigenstate of the measured observable), which in turn has no other physical meaning than that of a probability amplitude. Thus, in the usual interpretation, before the measurement the micro-object was in no definite state at all (and even it cannot be said to have existed), whereas in the causal interpretation, if I understand it correctly, the measurement act changes the system from one precisely defined and real state into another precisely defined and real state, although the former may not be known experimentally with sufficient accuracy. So that Heisenberg's relations are retained in Bohm's interpretation—as was to be expected since he does not change Schrödinger's equation ; but they are not *interpreted* as relations of complementarity, and this is just Bohm's point. Moreover, they are not conceived as eternal ; future theories, which are certainly badly needed, might change those relations, showing that they have a limited domain of validity. Rosenfeld misses this point because he identifies the uncertainty relations with their positivistic interpretation, namely, the doctrine of complementarity.

Nearly twenty years ago Castelnuovo, the distinguished mathematician, expressed an opinion that was then a warning and is still a lesson of prudence :

> It is always necessary to take into account that science will never be completed and that the progress of observations and of theory may lead from the present anti-deterministic phase to a deterministic phase

[1] Rosenfeld, *SC*, p. 393

—which, in turn, may be followed in the future by several similar alternatives.[1]

9 *Conclusions*

Our main conclusion is that the usual interpretation of quantum mechanics is neither the only possible rational one nor is it in agreement with scientific materialism. It is, on the other hand, consistent with the principles of logical empiricism, particularly with the Berkeleian theory of knowledge.

Not the least merit of the recent causal interpretations of quantum mechanics proposed by de Broglie,[2] Bohm,[3] Jánossy,[4] Novobatzky,[5] Takabayasi,[6] Vigier,[7] and others, as well as of the untiring criticisms of Einstein [8] and of Schrödinger,[9] is that they are awakening us from the dogmatic slumber (as Kant would say) in which most of us have been sunk. It has been proved in practice that complementarity is not the only possible interpretation of quantum mechanics. And this has made it possible to suggest several ways out of the present stagnation of theoretical physics, a stagnation which is certainly due in a large measure to the dogmatism with which the official philosophy of quantum theory has been maintained.

From now on no reflective theoretical physicist will have the right to utter words such as these :

> See how neatly the statistical predictions of quantum mechanics are adapted to the interpretation of the experimental results ! They are all susceptible to verification and they embody every single detail that experience reveals to us. Neither too much nor too little : what more do people want ?[10]

[1] Guido Castelnuovo, ' Il principio di causalità ', *Scientia*, 1936, **60**, 61-68, p. 68

[2] Louis de Broglie, *La physique quantique, restera-t-elle indéterministe ?* Paris, 1953

[3] Bohm, *IQT*

[4] L. Jánossy, ' The Physical Aspects of the Wave-particle Problem ', *Acta Physica Hungarica*, 1952, **1**, 423-467

[5] K. F. Novobatzky, *Annalen der Physik* (6), 1951, **9**, 406 ; 1953, **11**, 285

[6] T. Takabayasi, *Progress of Theoretical Physics*, 1952, **8**, 143 ; 1953, **9**, 187

[7] Jean-Pierre Vigier, *C.R. Acad. Sci.*, 1951, **233**, 1010 ; 1952, **234**, 410 ; 1952, **235**, 1107

[8] A. Einstein, B. Podolsky, and N. Rosen, ' Can Quantum-mechanical Description of Physical Reality be Considered Complete ? ' *Physical Review*, 1935, **47**, 777-780. See also Einstein's ' Autobiographical Notes ' and ' Reply to Criticisms ' in P. A. Schilpp (Ed.), *Albert Einstein : Philosopher-Scientist*, Evanston, Ill., 1949

[9] Erwin Schrödinger, ' Are there Quantum Jumps ? ' this *Journal*, 1952, **3**, 109-123, 233-242. See also *Endeavour*, 1950, **9**, 109-116 and *Scientific American*, 1953, **189**, No. 3, 52

[10] Rosenfeld, *SC*, p. 404

From now on more and more people want to understand formulae, not merely to calculate them ; from now on more and more people want to leave behind the present stagnation, more and more people are realising that this will be possible only by foregoing the positivistic philosophy which the doctrine of complementarity integrates. Their attitude will resemble more and more the free and fearless attitude which Harvey [1] described three centuries ago :

> True philosophers, who are only eager for truth and knowledge, never regard themselves as already so thoroughly informed, but they welcome further information from whomsoever and from wheresoever it may come ; nor are they so narrow minded as to imagine any of the arts or sciences transmitted to us by the ancients, in such a state of forwardness or completeness, that nothing is left for the ingenuity and industry of others. On the contrary, very many maintain that all we know is still infinitely less than all that still remains unknown ; nor do philosophers pin their faith to others' precepts in such wise that they lose their liberty, and cease to give credence to the conclusions of their proper senses.

[1] William Harvey, *Exercitatio Anatomica de Motu Cordis et Sanguinis*, Dedication (1628), *apud* W. T. Sedgwick and H. W. Tyler, *A Short History of Science*, New York, 1917, Appendix D

NOTES AND COMMENTS

Comments on ' Degree of Confirmation ' by Professor K. R. Popper

In a recent issue of this *Journal*, Professor K. R. Popper [1] expressed his misgivings of the current identification of the degree of confirmation of x by y with the (relative) probability of x given y (p. 143), insisting that his disagreement is not a verbal one (p. 146). I claim, and shall try to prove, that the issue *is* a verbal one and that Professor Popper's point has been fully anticipated and adequately been taken care of by at least one of the adherents of the criticised identification. The main point of this comment is, however, not so much the clarification of a misunderstanding as an expansion of what seems to me a highly significant remark of Professor Popper's on scientific methodology.

I

It seems that Popper was the first to use the expression ' degree of confirmation ' (or rather its German equivalent: *Grad der Bewährung*). He introduced this term in § 82 of his *Logik der Forschung* (Vienna, 1935). Though he did not define it there very clearly, he stressed its being different from logical probability. Professor Carnap, however, began using this term, perhaps as early as 1936, as the systematic explication for the pre-systematic explicandum ' logical (or inductive) probability '. But thereby he did not disregard the point Popper had in mind. On the contrary, in his most extensive treatment of this complex of problems, *The Logical Foundations of Probability*, Chicago 1950, he dedicated a whole chapter (viz. chapter VI: Relevance and Irrelevance) to a very thorough discussion of the concepts crucial to Popper's point, though under a different terminology. Though Popper's degree-of-confirmation and Carnap's relevance-measure are differently defined, there can be no doubt that their explicanda are identical. The following dictionary should help to clarify the situation:

Popper's terms	*Carnap's terms*
absolute (logical) probability	initial confirmation
relative (logical) probability	degree of confirmation
degree of confirmation	relevance measure
supports	is positively relevant to
undermines	is negatively relevant to
independence	irrelevance

[1] K. R. Popper, ' Degree of Confirmation ', this *Journal*, 1954, **5**, 143-149.

The case, regarded by Popper as crucial, in which a statement y undermines each of two statements x_1 and x_2 while supporting their conjunction, is explicitly discussed by Carnap (p. 394) as case 3*b*, and an illustration given on the following page. It is far from obvious why ' we should have to say in such a case that y confirms x_1x_2 to a higher degree than it confirms either x_1 or x_2 ', as claimed by Popper (p. 144), rather than describe the situation in the way done in the preceding sentence or say, in Carnap's terms, that in such a case (the evidence) y is negatively relevant to each of (the two hypotheses) x_1 and x_2 and nevertheless positively relevant to their conjunction.

It is possible that some authors confused confirmation with relevance. Carnap certainly was not one of them. It is indeed slightly awkward ' to say that evidence y of which x is completely independent can yet strongly " confirm " x ' (p. 145), and this way of speaking seems to diverge from ordinary usage, but it is not absurd or necessarily misleading. Carnap, at any rate, has not been misled by it.

2

I come now to my second point. In addition to the argument from ordinary usage, Popper has another objection against using ' degree of confirmation ' as synonymous with his ' (logical) probability '. He points out that methodologists of science are accustomed to say that science aims at hypotheses with a high degree of confirmation. However, the argument continues, ' *Science does not aim, primarily, at high probabilities. It aims at a high informative content, well backed by experience. But a hypothesis may be very probable simply because it tells us nothing, or very little.* A high degree of probability is therefore not an indication of ' goodness '—it may be merely a symptom of low informative content ' (p. 146).

Now, I agree fully with the spirit of Popper's remark, though not with the letter and certainly not with his belief that he thereby succeeded in uncovering a serious fault in the current theory of degree of confirmation. *Prima facie*, it seems indeed as if high informative content is incompatible with high probability. Since high informative content is certainly a desideratum for scientific hypotheses, it seems to follow that the scientists do not aim at high logical probabilities for them, and are not interested in obtaining for them a high degree of confirmation, in Carnap's sense, which sounds very strange indeed. The situation seems to be, however, simply this : A ' good ' hypothesis is one that has high *initial* (or absolute) informative content, hence a low *initial* confirmation, in Carnap's sense, but a high degree of confirmation, in Carnap's sense, relative to the total available evidence, hence—and this sounds admittedly slightly paradoxical—a low informative content, relative to this evidence. Though degree of confirmation and measure of informative content (as a matter of fact, there are many

plausible measures of informative content, some of which have been discussed recently by Carnap and myself in a common paper [1]) are, roughly speaking, inversely proportional, a ' good ' hypothesis must indeed have high degrees of both, with contradiction easily avoided through the qualifiers ' initial ' (or ' absolute ') and ' relative to the available evidence ', respectively.

Though the methodological situation may be adequately and satisfactorily described in terms of Carnap's degree of confirmation alone (informative content being definable on its basis), there is, of course, no objection to employing also the relevance terminology and saying, for instance, that good scientific hypotheses should have low initial information, hence high initial content, with empirical evidence (' experience ') being highly positively relevant to them.

Altogether, though Popper's recent polemic against the current theory of degree of confirmation seems to be unjustified and in spite of the fact that his positive proposals in this respect seem to have been effectively anticipated in Carnap's treatment of relevance, there can be no doubt that the complex position held by scientific hypotheses with regard to logical probability, informative content, and relevance of empirical evidence have been greatly clarified by Popper's remarks.

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' Content ' and ' Degree of Confirmation ' : A Reply to Dr Bar-Hillel

DR BAR-HILLEL's concluding remarks are gracious, and even encouraging. But since they do not quite match what he says in the body of his note, they are perhaps only meant as balm to my wounds. For if he is right in what he says in the body of his note then I cannot possibly have ' greatly clarified ' the ' complex position ' by my remarks. On the contrary, I must have left this position in a state of even worse confusion than it was left in by ' the current theory of confirmation ', as Dr Bar-Hillel calls it.

My note ' Degree of Confirmation ' which has given rise to Dr Bar-Hillel's comments was critical of Carnap's theory, because I had to make clear why I wanted to propose a new definition of degree of confirmation.

[1] Y. Bar-Hillel and R. Carnap, ' Semantic Information ', this *Journal*, 1953, **4**, 147-157, especially 149-151

I am afraid that Dr Bar-Hillel forces me now to criticise Carnap's theory further.

For the simple answer to Dr Bar-Hillel is that *there is no 'current theory of confirmation'*. Or, to put it more clearly, the theory proposed in Carnap's two books, *Logical Foundations of Probability*, and *The Continuum of Inductive Methods*, is partly inconsistent, and partly inadequate from the point of view of *his own* requirements, not merely from that of *my* requirements. Moreover, the points where it fails were briefly (but, I think, sufficiently) discussed in my *Logik der Forschung*, fifteen years before the 'current' theory was published.

I shall try to prove these charges as clearly and simply as possible.

Carnap sets out to establish a theory of confirmation. He distinguished three kinds of concept of confirmation :[1]

(i) the classificatory concept of confirmation ;
(ii) the comparative concept of confirmation ;
(iii) the quantitative or metrical concept of confirmation.

All three concepts are discussed at some length ; but in the end, only a theory of (iii) is offered.[2] It is this concept which I shall prove to be partly inconsistent and partly inadequate, by Carnap's own standards.

A very important touchstone of the theory is, clearly, the way it treats *the degree of confirmation of a universal law*. (In my opinion, it is the *only* important case ; I know that Carnap's opinion is different, but we need not discuss this point here.)

On page 571 of *Probability* Carnap tells us, quite correctly, that, according to his definition of degree of confirmation, *the degree of confirmation of all universal laws is zero* for a world with infinitely many individuals, and indistinguishable from zero for any world with very many individuals (especially if we know, at least, a very large lower limit).

Now this result is clearly counter-intuitive. It attributes to the best confirmed laws such as 'sugar is soluble in water' precisely the same 'degree of confirmation' as to laws which are always refuted (or which

[1] Carnap, *Logical Foundations of Probability*, 1949 ; here for short '*Probability*', see especially pp. 21–23 ; by the same author, *The Continuum of Inductive Methods*, 1952, for short, '*Methods*' ; and 'On the Comparative Concept of Confirmation', this *Journal*, 1953, **3**, 311 sqq. ; for short '*Comparative*'.

[2] *Probability*, p. 492, which says of the classificatory concept, 'This concludes the discussion of the classificatory concept. We have not found an adequate explicatum . . .' Similarly the later *Comparative*, p. 317 (in a reply to Dr Bar-Hillel's criticism): 'The definition of the comparative concept in my book was too narrow. . . .' Then Carnap quotes, p. 318, from *Probability*, p. 467 : 'However . . . it seems doubtful whether a simple definition can be found', and adds : 'The discussions in this article will make these doubts even stronger.' Thus no current theory of either the classificatory or the comparative concepts is claimed to exist.

are self-contradictory). And Carnap himself admits that this result is counter-intuitive: he says that an engineer will (intuitively) call certain laws 'very reliable'; 'well founded'; 'amply confirmed by numerous experiences'; and he adds himself that 'these phrases . . . are intended to say something about . . . degree of confirmation'.

But Carnap does not think that he has shown the *inadequacy* of his definition of degree of confirmation. Instead, he introduces *ad hoc* two new concepts, specially designed for the purposes of escape from the difficulty; the concept of an *instance-confirmation* and that of a *qualified instance-confirmation*. I shall criticise these two a little later; first I shall try to show why Carnap, by his own standards, should have discarded his definition of degree of confirmation as *inadequate*, as soon as he found a case which he himself felt to be counter-intuitive.

For we read on page 232 of *Probability*: 'How is the adequacy of a function c proposed as a [definition of degree of confirmation] . . . to be judged?' The answer contains the following passage: '. . . Then we examine whether the value of $c(h, e)$, calculated on the basis of the given definition, is sufficiently in agreement with the intuitive value.' And Carnap continues: 'Since the intuitive determination of a value is in general rather vague, an approximate agreement will be regarded as sufficient.' Clearly, there is not very much hope of proving inadequacy by this vague method. Nevertheless, the intuitive inadequacy we have found is sufficiently glaring to prove inadequacy. The gap between the intuitive value for a very well confirmed law and the value zero just could not be wider.[1] If ever there can be a case in which we can reject adequacy on the basis of the test suggested by Carnap, this must be it.

But I find in *Probability* a second remark of Carnap's on adequacy which, I think, clearly implies the inadequacy not only of the definition of degree of confirmation which he favours, but of any function which satisfies the traditional laws of the calculus of probability (and therefore of all 'regular c-functions' in Carnap's terminology).

The passage is connected with Dr Bar-Hillel's comments in several ways. First, I should not have noticed it without Dr Bar-Hillel's insistence that Carnap has anticipated my proof (in 'Degree of Confirmation') of the inadequacy of any probability function to serve as a measure of degree of

[1] In the continuation of the passage, Carnap mentions that if the inadequacy arises only in special cases 'the definition need not be entirely abandoned; it may be that a suitable modification can be found'. But what is meant here by 'modification' is clearly not the replacement of the definition, for these special cases, by a completely different definition, but a modification of the general definition which makes it more adequate for the special cases. (I personally should not admit that *all universal laws* form a 'special case'; but again, there is no need to argue this point.)

confirmation. But Dr Bar-Hillel is right and I am very grateful for his reference: Carnap has anticipated, in essence, my examples; he has even drawn from them the conclusion that certain classificatory confirmation-concepts are inadequate. But he has not drawn from his examples the conclusion which I have drawn: that all probability functions are inadequate to serve as degree of confirmation.

I shall not again go over the field covered by my previous note. But the following is an immediate consequence of his and of my examples:

Let the content of x be part of that of y, so that x follows from y. Then the following '*content-condition*' (Carnap calls it 'consequence condition') is *invalid*: 'If x follows from y, then every z that confirms y confirms x at least to the same degree as it confirms y.'

Carnap draws from the examples mentioned the correct conclusion that what I call the 'content condition' is invalid. But we have the following universally valid formula of the calculus of relative probabilities for every a, b, and c:

$$p(ab, c) \leqslant p(b, c)$$

now let ab be y, and let b be x, so that x follows from y, and that the content of x is contained in the content of y. Then we see that, for every z,

$$p(y, z) \leqslant p(x, z),$$

which is, precisely, the invalid content condition which Carnap uses on the bottom of page 474 of *Probability* as an argument to show the invalidity of a confirmation concept. But he does not say that it proves the inadequacy of all probability functions ('regular c-functions'), which I pointed out in my note—and which I had pointed out long ago in my *Logik der Forschung* where I used the invalidity of the content condition for the same purpose. (This partly answers Dr Bar-Hillel's point about contents.)

I think I have proved that on Carnap's own showing, his 'regular c-functions' and therefore his concept of 'degree of confirmation' are inadequate.

But I have still to say something about his two concepts of instance-confirmation; for these are not regular c-functions at all (a point not mentioned by Carnap), that is to say, of course, not regular c-functions of their arguments.[1] It might be thought, therefore, that they escape our argument from the invalidity of the content condition. This, however, is not the case, as Dr Bar-Hillel will easily verify.

But this is not my main argument against the two concepts of instance-confirmation. They are different.

(1) The concept of unqualified instance-confirmation is inadequate for at least two reasons. The first is that in a sufficiently complex world, its

[1] This fact may be easily overlooked by Carnap's readers, because the two concepts are defined with the help of regular c-functions.

value will be very close to zero for any complex predicate. The second is that it shares the absurdity of the (rectified) qualified instance-confirmation which will be discussed next.

(2) The qualified instance-confirmation of which Carnap says in *Probability*, page 572, that it 'seems in many cases to represent still more accurately what is vaguely meant by the reliability of a law *l*' is, I am sorry to say, inconsistent: it is hit by the paradox of confirmation (discussed in *Probability*, page 469), as Dr Bar-Hillel will no doubt see at a glance if he compares the two pages.

This means that not even a vaguely adequate degree of confirmation for a universal law has been proposed. But, as a matter of fact, the paradox of confirmation is not so serious as it looks: there is a general method for avoiding it by symmetrisation, and it can be easily applied to our case. (It means replacing, in (15) on page 573, 'h'' by '$j \supset h'$', and 'j'' by '$h' \supset j$'.) But even after this rescuing operation, the rectified concept of qualified instance-confirmation and the non-qualified concepts both lead to absurd consequences.

Take a universe of coin tosses with only two predicates: 'coming up heads' and 'coming up tails'.

Let somebody propose the hypothesis 'all tosses always come up heads'; let there be a sequence of tosses to test this hypothesis, with tails coming up on the average at every second toss. Then we will all say that the hypothesis has been amply refuted by the evidence. However, Carnap's unqualified, and the by me rectified qualified, instance-confirmations both give the hypothesis a confirmation value of exactly $\frac{1}{2}$.

Similarly, a hypothesis which is regularly refuted in every hundredth instance gets a confirmation value which rapidly approaches 99/100. I need not say that both ought to have zero confirmation.

I discussed an equivalent case long ago in my *Logik der Forschung*, page 191, and briefly described how these absurd consequences may lead the probability theorist to adopt another definition which, as I there showed, leads to the probability zero for all hypotheses. I was no prophet: I should have presented the problem the other way round, showing how one can escape from the frying pan of zero probability into the fire of instance-confirmation, with its absurdly high probabilities for regularly refuted hypotheses.

Thus there is no 'current theory of confirmation', I contend.

But what about my own theory, and Dr Bar-Hillel's claim that it has been anticipated by Carnap's relevance-measure?

As I pointed out in my note, both my concepts, 'explanatory power' and 'degree of confirmation', are *measures of dependence*; and so is every measure of 'relevance' (cf. Keynes' *Treatise*, pp. 54, 120, 121, 146, 150). This does not mean that Carnap's measure of relevance is adequate for a

measure of degree of confirmation. (It does not satisfy most of my criteria of adequacy nor my other *desiderata*.) Nor does Carnap anywhere propose that his relevance measure should be used as one of degree of confirmation. In fact, Keynes, or rather W. E. Johnson's [1] ' coefficient of dependence ' (or ' coefficient of influence ') which I may denote by ' *Co* ' is mathematically much nearer to my degree of confirmation since my function *E*, the explanatory power, is equal to $(Co - 1)/(Co + 1)$ (provided we change the order of Keynes' first two variables). I suspect, however, that both Johnson and Keynes would have been surprised to see their ' coefficient of dependence ' championed as a measure of the explanatory power of a theory, or of the degree to which a theory is confirmed by tests.

But had I done no more than show that Carnap's ' degree of confirmation ' is inadequate while his ' relevance measure ' was an adequate measure of the acceptability [2] of a theory (which it is not), I should have done something not anticipated by ' the current theory '.

I may now sum up my reply to Dr Bar-Hillel's points 1 and 2, by way of criticising his ' *dictionary* '.

Dr Bar-Hillel's dictionary is incorrect. As to its third line (which happens to be the point at issue) : what I call ' degree of confirmation ' is not the same as Carnap's ' relevance measure '. Carnap's relevance measure approaches zero with increasing content of the hypothesis (or of any other statement involved) ; and it becomes zero for every universal law. My ' degree of confirmation ' approaches one with increasing content of the hypothesis, provided it is a successful hypothesis. These are mathematical consequences of the two definitions. The discrepancy could hardly be greater. Also, Carnap leaves no doubt that from the point of view of his theory, any non-additive confirmation function is ' entirely unacceptable ' (cf. *Continuum*, p. 85 ; see also *Probability*, pp. 369 f.) ; but my ' degree of confirmation ' is non-additive.

Lines four to six of Dr Bar-Hillel's dictionary are also incorrect, for the same reason. ' Is positively relevant to ', in Carnap's sense, approaches zero, if the content of the statements in question increases ; while ' supports ', in my sense, behaves differently : a statement may have a very

[1] Cf. Keynes, *Treatise on Probability*, 1921, pp. 116, 150

[2] Carnap always intended (as I did) that his degree of confirmation should serve as a measure of the *acceptability* of a theory. Dr Bar-Hillel's historical comment, at the beginning of his point 1, may give the impression that the term ' degree of confirmation ' was from the beginning used by Carnap in a sense different from mine. The fact is, however, that Carnap's term ' degree of confirmation ' occurs first in ' Testability and Meaning ', *Philosophy of Science*, 1936, 3, where he used it to translate my term ' *Grad der Bewährung* ' ; cp. for example p. 427 where Carnap writes : ' Popper has explained the difficulties of such a frequency interpretation of the degree of confirmation ', etc.

high content and yet it may, for this very reason, strongly support or undermine another.

As to the remaining lines—one and two—they are correct. But they contain, precisely, the error exposed above : the content condition. Which means that, according to Carnap's standards as well as mine, ' degree of confirmation ' cannot be a probability function.

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REVIEWS

HAS HISTORY A MEANING?

It was in 1938 that Professor Popper conceived his book on *The Open Society and its Enemies.*[1] It was published in 1945 and a second and enlarged edition appeared three years ago. In the interval events of the greatest importance to mankind have occurred : the Second World War, the discovery of atomic energy, and the Chinese Revolution. We are still in the Cold War and great changes are clearly taking place in every part of the world. What now has Dr Popper to tell us of all this? His answer, delivered with great force and with a wealth of scholarship, is that history, now as ever, has no meaning and that we can do very little about it. Dr Popper who likes coining words might well describe himself as a *philagnoist* or lover of ignorance. But this attitude though a negative one is in no way passive. The book is an attack on philosophers of almost every school from Heraclitus to Heidegger for daring to think they knew something about man and society and for deluding themselves into beliefs that turned out to be dangerous illusions which have brought nothing but misery. His own contribution, the concept of Open Society, is mainly defined by negatives. It is dangerous to try to do good for we will almost certainly be mistaken as to what good is. The most we can do is to struggle against universally recognised evils. Even there we must avoid too wholesale an approach ; progress can only be achieved piecemeal.

The central enemy of Dr Popper's Open Society appears to be the belief in an historical process and the confidence in prophecy that goes with it. He finds himself in instinctive opposition to the two forms of social thought which he labels *utopianism*, the idea that we know what is best for human society, and *historicism*, the idea that knowledge of the laws of the past can help to predict the future. The difficulty is that he wishes to saddle both of his *bêtes noires*, Plato and Marx, with these linked errors. Now it is clear enough that Plato is a utopian but it is hard to make him an historicist without stretching the word so that every thinker can be accused of it. Similarly it is difficult to convict Marx of utopianism considering that his major contribution to the creation of socialism was purging it from all predetermined utopian schemes.

What makes the whole of *The Open Society and its Enemies* such strange reading is that the methods and the feelings of both of his antagonists have

[1] *The Open Society and its Enemies.* By Karl R. Popper. Routledge, London, 2nd edition, 1952. Two vols. pp. 318 and 375. 42s.

so deeply entered into Dr Popper's own mind that he appears much of the time to be struggling against himself.

His attack on Plato is largely in terms of Marxist historical materialism. He sees him as the product of the failure of the commercial imperialism of Athens. It is this that enables him to strip from Plato the aura of divine impartiality and to show him up as an old reactionary who was mainly concerned in finding some way of preserving by all means, fair and foul, the privileges of the superior upper classes. Dr Popper is even able to show how the central themes of Plato's philosophy, the ideal forms, were reflections of his hatred of political change and his resolve to substitute the Spartan tribal aristocracy for the democracy of Athens. It is not for me to criticise the justice of a picture which has certainly shocked many scholars but I feel that Dr Popper has done more credit to Plato than most of his admirers have by at least discussing what he was trying to do. But whether fair to Plato or not Dr Popper is right in seeing the influence of Plato in the authoritarian regimes of Fascism and Nazism. Indeed it is clear that the whole book was written under the threat of Nazism and in protest against the basic ideas of the divine *state*, of the leadership principle, and of the superiority of race, all of which received their most complete and persuasive description in Plato's *Republic*.

In his second volume Dr Popper is not so hard on Marx as he is on Plato, and the whole treatment is far more tentative and confused. His real fury is reserved for Marx's forerunner Hegel who provides the link between them and combines both their vices. For Hegel, according to Popper, in glorifying the Prussian state, is as reactionary as Plato and is responsible for the concept of necessary *progress* which was Marx's central error. This was all the more regrettable, in Dr Popper's view, as Marx would otherwise have been a great supporter of the Open Society fighting against the evils of his time and for freedom. Where he went wrong was in believing in the need for a revolution and a seizure of power by the working classes. Those evils could be, and in Dr Popper's view have been, largely removed in the course of time by peaceful and piecemeal social engineering.

This method, which is the only one on which Dr Popper relies, is a curious transformation into social terms of a profound misunderstanding of the nature of technical advance. Dr Popper apparently thinks that new machines evolve by a succession of slight improvements of the old. This is indeed the way which led, for instance, from the canoe to the galley or from the chariot to the stage-coach. But it is emphatically not the way in which major and significant advances have been made throughout the whole course of technical history. These have arisen from experiences and ideas derived from sources remote from their application and have been imposed on rather than grown out of the existing technology. Fire, gunpowder, the steam engine, the dynamo, the atom bomb are not

piece-meal improvements but radical innovations. As we approach modern times, we find such innovations as the fruit of a profound analysis of natural forces. It seems hardly likely that we shall get any significant improvement in social affairs by tinkering, as Dr Popper advises, with the social system but rather by an equally serious and scientific analysis of social matters.

Dr Popper does recognise even when he disagrees with him, that Marx attempted such an analysis. He has been at some pains to be fair to Marx and even to read him with some understanding, as his elaborate notes show. He even denounces much of the ignorant criticism of Marx which appears in much contemporary writing. For instance he dubs as *vulgar marxism* the view that the exploitation of the workers is due to some conspiracy of wicked capitalists. His own view of Marx differs almost as much from the original. We might call it for comparison *sophisticated marxism.* It starts with the belief that Marx's concept of *scientific socialism* was identical with *social determinism* and that his laws of social change should have enabled the future to be predicted with certainty. Dr Popper is then able to show to his own satisfaction that Marx's prophecies were not realised and consequently to dispose of him as a thinker while still objecting to him as a social force. Now Marx himself, as he shows in his books and letters, was no determinist; he never thought he could do more than indicate the general lines of social advance. As a scientist he knew only too well that though laws may be true their application must needs depend on a host of circumstances which it is impossible to determine in advance. Above all, Marx was aware that history was not determined independently of human desires and actions but rather with human actions as the essential factor ; hence his life-long political activity. He distinguished only between those periods, covering most of the past, where the development was a blind resultant of different human wills and those of the present and future where the general run of events would come more and more completely under overall conscious human control. Marx himself never thought for a moment, even if some of his followers did, that there was no need for exertion, that history could just take its course. This is, in fact, the attitude that Dr Popper rightly condemns. But this determinist historicism is not Marxism even though it may have passed as such with Austrian Social Democrats against whose influence Dr Popper has reacted. It was certainly not that sort of Marxism which guided Lenin. His praise of Lenin is qualified by the claim that his successful direction of the revolution was not along the lines laid down by Marx. But what matters is not what Dr Popper chooses to think of as Marxism but what is thought to be Marxism by those who are using it in building a new civilisation, already counting over a third of the world's population. The career of Chou En Lai should show how a deep study of Marxism is compatible, to say the least of it, with highly successful action in the most complicated and difficult situations. It seems a pity that

Dr Popper, in revising his first edition, did not include an analysis of Chinese development. If he had he would perhaps have found reasons to modify his views of historicism, for the success of the Chinese revolution is largely based on a deep understanding of the historical evolution of Chinese civilisation.

It is because he has too narrow an interpretation that Dr Popper has avoided seeing that the central thesis of his version of Marxism—the doctrine of increasing misery—far from demonstrating by its failure the non-fulfilment of Marx's predictions has instead amply confirmed them. Economic crises, two world wars, colonial oppressions and insurrections have filled the years since Marx's death ; while at the other pole the wealthy, despite every form of taxation, do not seem any poorer. Hundreds of millions of people have come to see in these miseries and luxuries the exploitation which Marx was the first to explain and to accept his account of it. It is not much use in these days trying to prove that logically they are wrong, when in fact the great events of the time seem to follow the lines Marx sketched out and when alternative explanations other than purely mystical ones can give no account of them.

Certainly Dr Popper cannot give any and indeed spends a large part of his book proving that no such account can ever be given, that there are no historic laws. His arguments against the possible significance of history are some of the weakest of the book. They turn, not on the specifically *historic* character of human or social history, but on its alleged complexity and the inevitably arbitrary selection of facts. Now this has nothing to do with the nature of *history* as such, but only with its *subject* in human society and the argument applies with equal force to every social science of predominantly unhistoric character—to sociology, economics, or even ethics. Perhaps Dr Popper would deny validity to all these, as sciences, but in this case it is misleading to label as *historicism* a belief in the possibility of a reasonable ordering of social facts. There is indeed a real sense in which historical sciences differ from all others but this has nothing to do with human society as such. Historical sciences, from cosmogony through evolution to political history, differ from other sciences because they describe in the first place irreversible processes and not just sections of a quasi-reversible or at least quasi-repeatable process such as are studied in physics or physiology. Experiment and prediction here have nothing of the precision and decisive character that they have in the exact sciences. Yet these essentially historic disciplines have been dignified with the name of sciences because through observation and analysis we know more of what is likely to happen, even in the most complicated of them, than if we had not observed or analysed. It is generally accepted, for instance, that there are laws of organic evolution ; Darwin is a scientist just as much as Newton. Even if such laws do not enable us to predict how any species will evolve they have their value in

ordering our picture of the relations between organisms. There is no *a priori* reason why there should not be laws for the equally unique but far more rapid and complex evolution of human societies. They must, however, be expected to be even more general and less precise than those of organic evolution and far more so than those of the evolution of rocks or stars. Marx claimed to have found and tested them as working hypotheses certain to be improved and superseded as any other laws in science. Like all other laws in science they are liable to criticism but it is not criticism merely to assert repeatedly, as Dr Popper does, that there are no laws in the development of human societies.

Indeed the predominant character of Dr Popper's work is negative and in the strictest sense obscurantist. It is not, of course, obscurantist in the ordinary sense, forbidding secular knowledge in the service of a higher revelation. Here Dr Popper is a true rationalist, condemning supernaturalism old and new. He embraces ignorance for ignorance's sake. To him views are delusive, they can easily lead to large errors, to oracular statement, and rank irrationalism. Let us rather focus, urges Dr Popper, with his characteristic philosophic myopia, on detail in analysis and piecemeal reforms in practice. We shall never know until we do them whether they will do good, but they cannot do much harm.

Where Dr Popper does venture further than this, as in his discussion on the state, he gets into difficulties from which he cannot extricate himself. To him the state has only what he calls a *protectionist* function, it exists to protect the freedom of the subject, to protect the weak against the strong. Here we recognise the true milk of nineteenth-century liberalism. But even Dr Popper realises that protectionism cannot be achieved in the existing world without state intervention to curb wealth and secure employment and this new activity of states, which he calls *interventionism*, is one which he considers necessary but dangerous. Incidentally Dr Popper's methods of logical analysis require the use of a formidable number of 'isms'. I have counted more than thirty, many of them of his own creation. They are nowhere defined, indeed Dr Popper deliberately refuses to define them, which gives a certain flexibility to the argument, though it would not please Professor Dingle.

The most striking feature of Dr Popper's whole treatise is the absence of reference to technical advance and its social effects. It is this, particularly in the last few centuries, that has given an irreversibly progressive character to human history, just as the evolution of finer methods of control of environment through sense organs and intelligence did to the mammals. Social evolution is as indubitable a fact as organic, and though different, equally calls for explanation. Now what Dr Popper is only beginning to realise is that the basic values of his Open Society—freedom, justice, and equality—however eternal they may be in the Platonic sense, require different means to

secure them in an era of scientific technology than those of the time of traditional handicrafts. How to do this remains the major problem which must be solved before an Open Society can be attained in reality. And it can hardly be solved by Dr Popper's piecemeal methods.

The present importance of technical evolution, with its intricate linking of scientific, economic and social relations, imposes the use of historical considerations for their understanding and control. To reject these and to go further and say that history has no meaning, is effectively to help those very forces of irrationalism which Dr Popper so deplores. The denial of order and significance in human affairs is an increasingly fashionable attitude in reactionary circles today. If history is just a tale told to an idiot, sound and fury find their justification. But if there is to be something better than surrendering to chaos the way to it will need finding through some kind of science of human development which we cannot exclude in advance. The two prerequisites to a tolerable humanity capable of realising the aims of the Open Society are *economic security*, that is at least enough to eat, and *peace*, or at least the removal of the danger of atomic annihilation. This necessarily implies an effective economic system all over the world and the control of ' international crime and mass murder ', in Dr Popper's words. Human co-operation will have to become greater and more conscious without sacrifice of the potentialities of the individual. Both mean an increase of social organisation in scale and intricacy.

Those who really wish for the Open Society, and they are to be found in the Marxist camp no less than in the Liberal, are striving to find the way to do this in practice and in theory. It is going to be a difficult task and a critical one, for a false step now may destroy all humanity. That is why we need, whatever our differences of formulation or emphasis, to understand each other well enough not to waste our energies in futile conflict among ourselves but rather to concentrate against the real enemies of the Open Society—human ignorance, cruelty, and greed.

J. D. Bernal

The Illusion of the Epoch: Marxism-Leninism as a Philosophical Creed. By H. B. Acton
Cohen & West, London, 1955. Pp. viii + 278. 18s.

Although there are already innumerable books on Marxism, Professor Acton's deserves a warm welcome. It combines a delightful lucidity and modesty with a mastery of historical detail, and it is concerned with an aspect of Marxism which has been comparatively neglected, namely, its technical philosophy and ethics (as opposed to its sociology and economics). A subsidiary purpose is to reveal various intellectual influences on Marx,

Engels, and Lenin, and Professor Acton does this very illuminatingly, both with lesser known continental writers and with Hegel, whose influence on the *details* of Marxism (besides his well known influence on its grand design) is strikingly brought out: for instance, Marx's transformation of Hegel's idea of the estranged mind into the idea of work which sustains a system which controls and impoverishes the worker, and of Hegel's idea of harmonious self-consciousness into the idea of work which is an extension of the worker's personality.

The book contains particularly good analyses of the notion of a change of degree or quantity turning respectively into a change of kind or quality, and of the notion of an ideological superstructure determined by its economic basis. In his analysis of the former, Professor Acton brings out the important and puzzling point that dialectical materialism, which is of course deterministic, emphasises against mechanical materialism those very things which seem most incompatible with determinism—the leaps which occur in natural processes when some novel feature (such as consciousness) emerges. In his analysis of the latter notion Professor Action shows that the vague concept of 'productive relationships' (or 'economic basis') necessarily involves those factors of law, organisation, and morality which are supposed to belong to the 'ideological superstructure'. This explains an apparent obviousness about the claim that changes in the economic basis cause changes in the superstructure by revealing that the two go hand in hand—not because there is a causal connection but because the one kind of change *means* the other kind of change.

Professor Acton's main criticism is that Marxism contains a fatal inconsistency: it is a positivist creed based on a grandiose metaphysics. Now this kind of self-rebuttal is a recurrent characteristic of social philosophies, and I am not convinced that it is the most serious flaw in Marxism. It would certainly not alarm a Marxist dialectician who holds that nature is contradictory anyway.

My other slight dissatisfaction with Professor Acton's book is that he sometimes fails to draw his threads together. For instance, one finds scattered over many pages the following tenets of Marxism: (1) dialectics is a comprehensive science of the development of nature, human society, and thought; (2) to have scientific knowledge of something is to transform or make or control it; (3) the administration of the socialist state will be a permanent union of theory and practice. When these propositions are paraded together it is easy to infer from them that in the socialist state men's social arrangements and thoughts will be permanently controlled; but Professor Acton never quite draws this inference.

The book ends with a discussion between the author and an imaginary reader who begins as a fair-minded, somewhat ingenuous chap but develops into a demon catechiser who asks questions like, 'Are there any signs in

Stalin's answers to the questions put to him about the linguistic theories of J. Y. Marr of any radical change in the Materialist Concept of History? '

An excellent book.

J. W. N. Watkins

Problems of Consciousness: Transactions of the First, Second, Third, Fourth Conference (4 vols.). Edited by H. A. Abramson.
Josiah Macy Jr. Foundation, New York, respectively, 1951, pp. 200, $3; 1951, pp. 178, $3.25; 1952, pp. 156, $3.25; 1954, pp. 177, $3.25

The Josiah Macy Jr. Foundation has performed a valuable service in providing opportunity for discussions between eminent specialists from diverse fields with some common interest. Papers were followed by discussions taken down verbatim.

The present series challenges comparison with the series on cybernetics. The several transactions on that subject were stimulating and well directed. The present series has contributions by equally eminent thinkers, some of whom indeed were members of the cybernetics group. But this series is disappointing by comparison. In the other series those taking part knew what they were supposed to be discussing. Here this was not so. Definite problems hardly emerged. Various members of the conference described numerous states of consciousness and discussed physiological conditions for its occurrence and changes. Only at the end was the specific question of the rôle of consciousness raised by Professor Piaget.

In the first volume an attempt was made to arrive at some clear conception of ' levels ' of consciousness—not that there was any serious expectation that the aim would be achieved, but the existence of an aim gave some rough focus to the discussion. Professor Kleitman opened on ' The Sleep-Wakefulness Cycle ', and stressed the relation between degree of consciousness or variation of alertness and body temperature which affects brain activity. Dr Zilboorg described features of the psycho-analysis of dreams —presumably to depict another level of consciousness. Professor Beecher reported some interesting facts about pain, bringing out, for instance, the difference between having a pain and minding it. Dr Margaret Brenman discoursed interestingly on hypnosis—presumably to describe yet other states of consciousness. Finally Professor Hoagland returned to the beginning by discussing the ' Chemistry of Time '. His experimental work suggested that consciousness of time-flow depends on the rate of chemical action, which he also related to temperature. Thus he found a ' physiological clock ' in chemical processes. He was led to an interesting explanation of the sense that much (or little) time has elapsed: if there is high

temperature and much chemical or physiological action, subjective time is increased, which makes clocks seem slow, and so on.

In the second volume Dr Rapaport gave ' A Psychopathological and Psychodynamic View ' ; Dr Zilboorg discussed ' Variations in the Scope of Awareness ' ; and Dr Wright described ' Variations in States of Awareness in Schizophrenic Patients '. It is difficult to discern any unity in this volume or to become at all clear about what was being discussed.

At the Third Conference Professor Kety discussed ' Consciousness and Metabolism of the Brain ' ; he was concerned with physiological conditions that influence consciousness. Dr Wolberg described some further interesting features of hypnosis. And Dr Monnier described some experimental investigations on sleep. Again it would be hard to say what thread holds the volume together ; again attention was never focused on a specific problem.

The symposiasts became conscious that something had gone wrong. They appear to have noticed less the absence of problems than the heterogeneous nature of the discussions ; for the fourth volume opens with a valiant attempt to summarise the earlier transactions. It does not succeed ; it could not succeed, because it is impossible to summarise a large number of heterogeneous comments made without a goal in sight. This volume is notable, however, for a brief statement by Professor Talcott Parsons of his theory of ' action ', and for a further brief statement by Professor Paiget of some of his researches—and especially for his attempting to discuss a definite problem. My impression—though this may be rooted in personal predilections—is that the first and fourth transactions are better than the second and third.

Despite its defects, however, the series should be consulted. Many readers will find interesting odds and ends that will be new to them, ranging over an enormous field. The series is like the former London markets where lucky finds were to be had from time to time ; a note should be made of it by the reader who likes to browse in such haunts.

J. O. Wisdom

Science and Religion: A Changing Relationship (The Rede Lecture). By C. A. Coulson.
Cambridge University Press, 1955. Pp. 36. 2s. 6d.

This is a sermon to the text ' science is a language, one of the languages in which God can be described ', which is stated twice. The author regards it as ' false to the true character of science to distinguish secular and sacred parts of nature ', but thinks that ' we had better be careful how we enlist the aid of the scientist in justifying the ways of God to man '. He regards

a scientist's concepts as ' figments of his own creative genius ' and thus as similar to those in religion which are not ' publicly knowable in any direct fashion '. According to the author the modern scientist lacks a ' sense of immediacy ' in his relation to nature. Coulson attributes this to the inaccessibility of the observed objects ; the atomic physicist is ' screened off from what is happening by thick layers of concrete '. Consequently the author sees in modern science ' respect and excitement, sometimes astonishment, but all too seldom reverence '. He seems to identify his deity with the material universe for he speaks of ' the reality which the scientist discusses and which the Christian calls God '. Science, he claims, is leading us ' to rediscover the sacramental value of nature, as somehow holding God, and fulfilling part of His being '. ' The wave equation and the gene both speak to us of the nature of God '. Professor Coulson seems to give the credit for these conclusions to scientists who, he says, ' have forced us to think and illuminated our very thoughts '. I wonder.

This pamphlet has, perhaps, a place in moral philosophy. It does not, and is probably not intended to, contribute anything to those problems that are generally recognised as belonging to the philosophy of science.

REGINALD O. KAPP

L'Age Nouveau, No. 86. Edited by Jean Roussel
Marcello-Fabri, Paris, March 1954. Pp. 111. Frs. 160

THE MAIN contents of this number of *L'Age Nouveau*, a monthly periodical, consist in nine short independent essays on the theme *L'individu devant la science*. The essays are written from a variety of view-points, physical, biological, medical, psychological, and legal. As is to be expected where no single, specific question is raised (or raisable?), the approaches to the theme differ widely, and some of the contributions are little more than factual summaries. For readers of this *Journal*, the following articles may be mentioned as most likely to be of interest: L.-J. Delpech's essay on Cybernetics, in which the view taken is that conscious processes exercise an indispensable steering and controlling function; J. O. Wisdom on the concept of the body-schema, who discusses different interpretations and developments of this concept as employed by Head, and suggests that reference to it is helpful in explaining what is meant by ' person '; lastly, Aurel David's essay on the implications of the legal conception of personality, in relation to advances in surgery and grafting-techniques which make possible the passing of ' spare parts ' to new ' owners '. Other articles include one on Biology by Maurice Caullery, and one by René Piedelièvre and Etienne Fournier written from a medical viewpoint.

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RECENT PUBLICATIONS ON THE PHILOSOPHY OF SCIENCE

(*a*) Books Received for Review

Inclusion of books in this list does not preclude their being reviewed in later issues

Isaiah Berlin, *Historical Inevitability*, Oxford University Press, London, 1954, pp. 78, 6s.

A. J. Berry, *From Classical to Modern Chemistry*, Cambridge University Press, 1954, pp. xii + 251, 25s.

Max Black, *Problems of Analysis*, Routledge & Kegan Paul, 1954, pp. xi + 304, 21s.

George S. Claghorn, *Aristotle's Criticism of Plato's 'Timaeus'*, Martinus Nijhoff, The Hague, 1954, pp. xi + 149, 9.50 guilders.

Maurice Cornforth, *Dialectical Materialism: Vol. III, Theory of Knowledge*, Lawrence & Wishart, London, 1954, pp. 240, 10s. 6d.

Herbert Dingle, *The Sources of Eddington's Philosophy*, Inaugural Lecture, Cambridge University Press, 1954, pp. v + 64, 3s. 6d.

Herbert Feigl and May Brodbeck, *Readings in the Philosophy of Science*, Appleton-Century-Crofts, Inc., New York, 1953, pp. ix + 811, $6.00.

John R. Gregg, *The Language of Taxonomy*, Columbia University Press, New York, 1954, pp. ix + 70, $2.50.

C. C. L. Gregory and Anita Kohsen, *Physical and Psychical Research: An Analysis of Belief*, The Omega Press, Surrey, 1954, pp. 213, 15s.

A. W. Haslett (Ed.), *Science News*, Penguin Books, Harmondsworth, Middlesex, 1954, pp. 128, 2s.

Mary B. Hesse, *Science and the Human Imagination*, SCM Press, London, 1954, pp. 171, 12s. 6d.

Karl Jaspers, *Psychologie der Weltanschauungen*, Springer-Verlag, Berlin, 1954, pp. xix + 486, DM. 29.80.

G. O. Jones, *Boundaries of Physics*, Inaugural Lecture, 1954, Queen Mary College, pp. 22.

André Lamouche, *Le principe de simplicité dans les mathematiques et dans les sciences physiques*, Gautier-Villars, Paris, 1954, pp. 481, 1,800 fr.

John Lewis, *Marxism and the Irrationalists*, Lawrence & Wishart, London, 1955, pp. 141, 8s. 6d.

A. A. Luce, *Sense without Matter or Direct Perception*, Thomas Nelson & Sons, Edinburgh, 1954, pp. viii + 165, 12s. 6d.

Ernest Campbell Mossner, *The Life of David Hume*, Thomas Nelson & Sons, Edinburgh, 1954, pp. xx + 683, 42s.

Ernest Nagel, *Sovereign Reason*, The Free Press, Illinois, 1954, pp. 315, $5.00.

The Nuffield Foundation Ninth Report, University Press, Oxford, 1954, pp. 141.

The Nuffield Foundation : Report on Grants, 1943–53, University Press, Oxford, 1954, pp. 319.

Roberto Pavese, *Compendio di logica sintetica*, Cedam-Casa Editrice Dott. A. Milani, Padova, 1954, pp. 89, L. 800

Dagobert D. Runes (Ed.), *Treasury of Philosophy*, Philosophical Library, New York, 1955, pp. xxiv + 1,280, $15.00

Sir Charles Sherrington, *Man on his Nature*, Penguin Books, Harmondsworth, 1955, pp. 312, 2s. 6d.

O. L. Zangwill, *Psychology as the Study of Behaviour*, Inaugural Lecture, Cambridge University Press, 1954, pp. 36, 2s. 6d.

(*b*) ARTICLES

Ethel M. Albert, Symposium, 'Causality in the Social Sciences', *The Journal of Philosophy*, 1954, **51**, 695

F. H. Cleobury, 'The Bearing of Relativity on the Controversy between Realism and Idealism', *The Philosophical Quarterly*, 1954, **4**, 302

Lewis S. Feuer, Symposium, 'Causality in the Social Sciences', *The Journal of Philosophy*, 1954, **51**, 681

J. N. Findlay, 'The Logic of *Bewusstseinslagen*', *The Philosophical Quarterly*, 1955, **5**, 57

Gerald Holton, 'On the Duality and Growth of Physical Science', *American Scientist*, 1953, **41**, 89

I. P. Howard, 'A note on the Design of an Electro-Mechanical Maze Runner' *The Research Review*, 1953, No. 4, 54

W. Kroebel, 'Kant und die moderne Physik. Die kritische Naturphilosophie und ihre Beziehung zu den naturphilosophischen Problemen der Gegenwartsphysik', *Studium Generale*, 1954, **7**, 524

L. A. Lezama, 'General Theory of the Entities of Physics endowed with Dimension', *Episteme*, 1954, **7**, 29

Mario Lins, 'Functionalization of the New Logico-Conceptual Forms', Paper presented at the Second Brazilian Congress of Philosophy, 1953

Mario Lins, 'Integration of Theory and Research in Sociology', Paper presented at the First Brazilian Congress of Sociology, June 1954

Mario Lins, 'The Logico-Systematic Structure of Science', Paper presented at the International Congress of Philosophy, August 1954

Mario Lins, 'Perspectives for the Logico-Conceptual Integration of Science', Paper presented at the International Congress for the Philosophy of Science, August 1954

Mario Lins, 'The Social Frames of Reference', Paper presented at the XVIth Congrés de l'Institut International de Sociologie, September 1954

A. W. Phillips, 'Stabilisation Policy in a Closed Economy', *The Economic Journal*, 1954, **64**, 290

Andrew Pikler, 'Optimum Allocation in Econometrics and Physics', *Weltwirtschaftliches Archiv*, 1951, **66**, 7

Andrew Pikler, 'The Quanta-Kinetic Model of the Monetary Theory', *Metroeconomica*, 1954, **6**, 72

O. L. Reiser, 'The Field Theory of Matter in a Pantheistic Cosmology', *Scientia*, 1954, **48**, 1

J. J. C. Smart, 'The Relevance of Modern Analytic Philosophy for Science (I)', *The Australian Journal of Science*, 1954, **16**, 165

Angela M. Colantoni Stevani, 'Emile Meyerson e il problema dell'identico e del diverso', *Sophia*, 1955, **23**, 82

Meetings of the Philosophy of Science Group

The following meetings were held, by kind permission, in the Joint Staff Common Room, University College, London, during 1954-5.

1954

11 October : Address by the Chairman, Mr L. L. Whyte, 'One Way Processes in Physics and Biology'.

15 November : Mr E. C. Cherry, 'Information Theory'.

13 December : Professor C. H. Waddington, F.R.S., 'The Philosophic Bearing of some Recent Ideas on Evolution'.

1955

24 January : Dr J. T. Davies and Professor H. Dingle, Symposium on 'The Age of the Universe'.

14 February : Professor D. J. O'Connor, 'Determinism and Predictability'.

14 March : Annual General Meeting.
Mr B. A. Farrell, 'Scientific Method and Understanding of Persons'.

25 April : Professor Max Black, 'Wittgenstein's *Tractatus*'.

16 May : Dr G. Burniston Brown, 'Unification of Macroscopic Physics'.

20 June : Mr D. le Vay and Dr J. F. Tait, 'The General Adaptation Syndrome : the Concept and its Implication'.

ERRATA

In 'Two Autonomous Axiom Systems for the Calculus of Probabilities' by K. R. Popper in No. 21 (May 1955) :

p. 52, line 1 : for the first occurrence of '1.1' read '1.1^a'.

p. 52, line 3 : for '1.2' read '1.2^a'.

p. 55, line 1 : insert 'the smallest' before 'systems'.

p. 55, line 8 should read : $p(x, y) = \lim p(x_n, y_n)$,

p. 56, axiom D.1 : for the last occurrence of 'y' read '$\bar{y}$'.